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PROGRESS AND ENTREPRENEURSHIP

RANDALL G. HOLCOMBE

Economists have been studying the factors that improve people's material well-being at least as far back as Adam Smith (1776), but the phenomenon of economic progress–or as it is often more narrowly studied, growth–barely predates Smith.[1] Prior to about 1750, economic progress was so slow that people would have to be very observant to see any progress during their lifetimes. Everywhere in the world, the standard of living and the quality of life was much the same in 1750 as it was in 1650, and it was much the same in 1650 as it was in 1550. Indeed, it was much the same in 1550 as it was in 550.[2] Since then, economic progress has manifested itself partly in income growth, but even more in new methods of production and in new types of output. This article examines the indispensable role that entrepreneurship has had in the production of economic progress. The link between entrepreneurship and progress may seem obvious, yet the connection between the two in economic analysis is tenuous, partly because mainstream economics does not do a very good job of representing entrepreneurship or progress.

RANDALL G. HOLCOMBE is the DeVoe Moore Professor of Economics at Florida State University. The author gratefully acknowledges helpful comments from two anonymous referees of this journal.

[1]Prior to Smith, the mercantilists were interested in this same question and thought that a nation's accumulation of gold and silver increased its wealth, and the physiocrats argued that wealth is the product of the land, so encouraged agriculture. Thus the issue was raised before Smith's monumental treatise was published, and one of Smith's motives for writing *The Wealth of Nations* was to try to correct what he perceived as errors in the views of these earlier economists.

[2]See Galor and Weil (2000, p. 808) for estimates that per capita economic growth began only after 1500, and was very modest until about 1820.

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Economists tend to represent economic growth as growth in the level of income, and one theme here is that economic progress is much broader than that: To focus on income growth is to ignore the most important elements of economic progress. The changes in types of output and methods of production that create economic progress are the result of entrepreneurship, but entrepreneurship is rarely represented in models of economic growth. In the twentieth-century economic theory of the firm, firms are run by managers who choose the optimal levels of inputs and outputs so their firms can produce efficiently. Managers are not entrepreneurs. This article emphasizes the differences between growth and progress, and between management and entrepreneurship, to try to illuminate how entrepreneurship in firms creates economic progress. This, in turn, suggests a promising direction for a further development of the theory of the firm.

If one wants to use economic analysis to understand how human welfare has improved over time, and how it can continue to be improved, then the analysis must focus on progress, broadly defined, rather than narrowly on income growth. Because progress is the result of entrepreneurship, an economic analysis of progress must incorporate entrepreneurship. Economic analysis has often analyzed economic progress only in terms of income growth, so a good place to start is with a discussion of the concept of economic progress.

Growth versus Progress

Modern economic progress began in the eighteenth century with the industrial revolution, however economic progress was greater in the nineteenth century than the eighteenth, and greater in the twentieth century than the nineteenth.[3] This progress was generated by entrepreneurship, and the economic environment continues to be increasingly favorable to entrepreneurship, so there is every reason to think that economic progress will continue to accelerate in the twenty-first century.[4] Economic progress is often measured in terms of income growth. In the United States, per capita GDP was nearly seven times greater at the end of the twentieth century than it was at the beginning; yet to summarize economic progress by looking only at the growth in

[3]See Heilbroner (1970) for a persuasive discussion about the importance of the development of factor markets in the industrial revolution. Landes (1998) also takes an interesting look at the institutional foundations of modern prosperity.

[4]This conjecture about future progress is based on the idea that entrepreneurial actions generate additional entrepreneurial opportunities, as described in Holcombe (1998). There is the threat that growing government could stifle economic growth. For an analysis of this issue, see Ikeda (1997). Baumol (2002) conjectures that innovative activities are labor-intensive, and as the relative price of labor rises that may slow innovation and growth. Yet in the end, Baumol is optimistic about the economic prospects for the future.

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the value of economic output seriously misrepresents the nature of the economic progress that took place in that century.[5]

At the beginning of the twentieth century only about 1 percent of American households had cars; by the end of the century 91 percent of households had them. Largely because of advances in medical technology, life expectancy rose from 47 years at the beginning of the century to 77 years by the century's end.[6] Telephones were rare at the beginning of the century, but commonplace by the end of the century. Information acquisition and entertainment were completely transformed in the twentieth century. At the beginning of the century there were no movie theaters, no radio broadcasts, and no television. By 1900 electricity was available to some, and was used mainly for lighting, but by 1950, electricity powered radios, electric washing machines, and refrigerators. By 2000, most people classified as poor in the United States had indoor plumbing, air conditioning, telephones, and automobiles. The Internet revolutionized communication and allowed business ventures to span the globe. While only a few computers existed in the world in 1950, many people had more than one computer in their homes by 2000. Computers did not become common until the 1980s, and the World Wide Web did not exist until the 1990s. The first airplane had not yet flown at the beginning of the twentieth century, but by the end of the century travel throughout the world in jet aircraft was commonplace. Despite the tremendous GDP growth over the twentieth century, when one reflects on economic progress over the century, it is apparent that the primary component of economic progress is not the amount of income growth, as impressive as it was, but rather the substantial change in the qualitative nature of the economy's output.

At the beginning of the twentieth century the average work week in the United States was about 50 hours, and by the end of the century it had fallen to about 35 hours. Again, this quantitative change in hours worked, while impressive, does not reflect the changing nature of work, which became less dangerous and less physically demanding. People worked more with their minds and less with their bodies by the end of the century, and this is reflected in the fact that at the beginning of the century only 22 percent of adults had completed high school, while by the end of the century 88 percent had at least a high school degree. Accidental deaths, including those on the job, fell from 88 per 100,000 to 34 per 100,000 over the course of the century.

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[5]In addition to the concepts of growth and progress, one might also look at the closely related concept of development. Economic development tends to refer to the advancement of an economy from less-developed or developing toward the status of developed, whereas progress suggests the idea that already-developed economies (in addition to less-developed economies) can continue to advance. The same entrepreneurial elements that promote progress also can initiate development. Good examples of work on development along these lines are Bauer (1972) and Osterfeld (1992).

[6]Data in this and the following paragraphs are from Moore and Simon (1999, p. 6). See also Cox and Alm (1998, 1999), who show measures of economic progress beyond just income growth.

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Again, while people work fewer hours for more income, the more significant element of progress in the work people do is not the quantitative reduction in work hours or increase in output, but rather the qualitative changes in the nature of work. At the beginning of the century the reward for work was money, and most jobs were mainly manual labor. While money was still a primary motivation at the end of the century, people considered the pleasantness of a job, including intellectual stimulation, challenges, and workplace amenities as significant rewards for employment. Many people enjoy the work they do: something that would have been much rarer in 1900, when work was often physically demanding, dangerous, and tedious. One can look at growth in terms of increased output per hour of work, but the progress in terms of qualitative changes at the workplace is at least as significant as the quantitative growth.

This type of economic progress would only have been visible to people after the Industrial Revolution. The advent of factory production, and the use of steam power in manufacturing, transformed the nature of work, and allowed substantial improvements in productivity. By 1850 the range of manufactured goods available to people was unprecedented, and by 1900 the use of steam power to drive the railroads and waterborne shipping had revolutionized transportation. People's income levels increased throughout these 250 years in nations that had market economies, but even more striking than the growth in income has been the change in the nature of the types of goods and services people consume. The changes in lifestyles of the typical citizen, the types of goods and services consumed, and in the way that goods and services are produced, is progress.

The economic growth over this 250 year period has been remarkable, but the economic progress that has taken place over the same period is even more remarkable. Despite this remarkable progress, economic analysis of these changes tends to focus on the growth in the level of output rather than the changes in the nature of the output.[7] This results in a limited understanding of both the causes and the effects of economic progress. The effects have already been considered by noting that the changes in the types of goods and services consumed and in the nature of work are more remarkable than the growth in the amount of consumption. The causal factors are more important, however, because the changes in both the nature of output and the processes of production are what have caused this remarkable economic growth. Put

[7]Johnson (2000) gives a good account of the progress made in agriculture over a period of centuries, but still couches economic progress in terms of increases in per capita income rather than changes in the types of goods produced and consumed. Similarly, Galor and Weil (2000) discuss the substantial economic progress after 1700 made possible by technological innovation, but depict that progress as income growth rather than qualitative changes in output. Both of these articles appeared in the *American Economic Review*, typically considered the top academic economics journal, suggesting mainstream economic thinking on the topic.

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plainly, the quantity of output would not have grown as it has without changes in the nature of both the processes of production and the type of output being produced.

The growth of the automobile industry in the twentieth century illustrates the importance of changes in both production processes and the types of goods produced. Economic progress meant enhancing people's transportation options by making automobile travel available to a large segment of the population, changing the type of output. Assembly line production allowed a substantial increase in the output of automobiles per worker. But focusing on the growth in output per worker obscures the more important fact that the types of goods produced, and the way they were being produced, had been substantially transformed within that span of a century.

Progress in one area often leads to progress in others, as Holcombe (1998) notes. The widespread availability of the automobile has changed the way people shop. Supermarkets, shopping malls, and large discount stores would not be feasible if people could not drive their own cars to transport substantial quantities of goods. The corner grocery, while just a convenient walk away, was more expensive and had less variety. The supermarket can draw from a larger customer base because more people are within driving distance than are within walking distance. Customers can buy more each time they shop, because they can carry more in their automobiles than they could transport without them. For many reasons, supermarkets can offer a greater variety of goods at lower cost. The same is true of discount stores like Wal-Mart, and these innovations in retailing would not have been possible without innovations in personal transportation.

New modes of shopping include not only larger, more centralized stores but also catalog shopping and now Internet shopping. These innovations have also led to increased product variety by, to use Smith's (1776) phrase, expanding the extent of the market. The advent of long distance telephone calling—and the steep decline in its price—made it feasible for potential customers to call sellers thousands of miles away to order products immediately, and the sharp decline in transportation costs made it feasible to ship individual purchases thousands of miles to buyers. This allows greater variety in the types of goods and services offered for sale, and gives entrepreneurs an incentive to introduce products that might have an insufficiently small market if they were just sold in one town, but have a large enough market if they can be sold nationwide, or even worldwide. Progress in one area leads to progress in others.

There are limits to the utility of consuming increasing quantities of currently-produced goods, which limits potential growth. Although income grew by about seven times in the twentieth century, Americans in 2000 would have found limited utility in having seven times as much food as Americans in 1900. But there are no limits to progress, because there are no limits to the types of new goods and services that people might benefit from consuming.

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Growth without progress is self-limiting, whereas progress brings with it growth.

Economic growth is a minor component of economic progress, but the changes that will allow progress to continue are far from obvious. It takes insight to perceive changes that might alter the nature of production processes, or that might lead to the introduction of new goods and services. Without people who have these insights, and who act on them, economic progress would not take place. Perceiving and acting on such a profit opportunity is entrepreneurship, which is the engine of economic progress. Economic analysis has tended to focus on growth–the production of increasing amounts of output–so it is important to see the distinction between growth and progress, and to see that in the long run, progress brings with it growth, but growth does not necessarily imply progress.

Entrepreneurship versus Management

The methods of modern economics tend to overlook the significance of entrepreneurship in creating economic progress. Economic theory focuses on the properties of economic equilibrium, and in the theory of competitive markets, actions of firms are completely constrained by the parameters of the market. Firms are assumed to maximize profits, and in a competitive equilibrium, profits are competed down to zero, so firms must choose the optimal mix of inputs and combine them as efficiently as possible in order to minimize costs to remain in business. In a typical formal presentation of the theory of competition, the firm buys inputs of capital, K, and labor, L, which are combined in a production function to produce output, Q, so $Q = f(K,L)$. The firm must pay interest at rate r to hire capital and wages at rate w to hire labor, so the firm's costs are $rK + wL$, and its revenues are the price of its output, p, times the quantity sold, or pQ. The firm's profit, Π, is its revenues minus costs, so $\Pi = pf(K,L) - rK - wL$. Within this framework, the only thing the firm can choose is its levels of K and L, so profit maximizing means choosing the levels of inputs that maximize profit. Profit is maximized by hiring inputs up to the point where their marginal products are equal to their prices.

People who run the competitive firms in this framework are managers who have as their goal producing the optimal amount of output at minimum cost. This entails choosing the right combination of inputs, and as Alchian and Demsetz (1972) note, also entails seeing that inputs are used as productively as possible. Workers need to be kept from shirking, and more generally, inputs must be used effectively to maximize profit. Good management means efficiently combining inputs to produce output. With good management the firm will maximize its profits; with poor management it will not, and will be competed out of business by more efficient firms.

In an economy characterized by economic progress, this type of management will be insufficient for a firm in a competitive market to survive. Over

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time, entrepreneurs uncover new and more effective ways of combining inputs and outputs, so the manager who simply prevents shirking and waste will be left behind by firms that adopt new production processes. More significantly, entrepreneurs will discover new types of output that will better satisfy consumer desires, so the manager who keeps producing the same type of output will be left behind by firms that offer more innovative products. Examples can be seen throughout the economy, in instances as varied as changes in fast food restaurant menus and food preparation methods, new styling and features in automobiles, and of course, in the substantial advances in computer and communication devices. As Boudreaux and Holcombe (1989) note, this entrepreneurial theory of the firm is substantially different from the neoclassical model that serves as the foundation for mainstream economics. One key difference can be found in the assumptions underlying these two approaches to analyzing economic competition. Consider two areas of entrepreneurship—finding more profitable methods of production, and discovering better product characteristics for consumers—within the standard competitive model.

The discovery of more profitable methods of production might arguably be able to be represented in the standard competitive model, by noting that there may be alternative methods of production, g, such that $g(K,L) > f(K,L)$. Framed in this way, the shortcomings of the standard approach are apparent. While the firm's management can choose levels of K and L from a continuous scale to solve the maximization problem, there is no menu of production functions f, g, h, etc., that offer alternatives from which the firm can choose. After the fact one might represent a new production technique as a different production function, but the theory offers no hints as to whether an alternative production function exists, and if it does, how a profit-maximizing firm might go about discovering it. Entrepreneurship, represented this way, is exogenous to the model.

The discovery of new and improved products and product characteristics is even further outside the model, because it is assumed away. In the standard model, firms in a competitive industry are assumed to produce a homogeneous product, and any deviation from this assumption is a movement away from competition toward monopoly. In contrast, an entrepreneurial view of product homogeneity sees product differentiation as a competitive strategy designed to try to gain market share and produce a competitive advantage. The managerial view of competitive markets sees product differentiation as anticompetitive, whereas the entrepreneurial view sees product differentiation as an integral part of the competitive process.

People who run firms serve both managerial and entrepreneurial functions.[8] As managers they try to minimize costs by choosing the optimal levels

[8]Baumol (1993, pp. 2-5) makes this same distinction between entrepreneurship and management, noting that entrepreneurs often also undertake management activities, and emphasizes the importance of entrepreneurship to economic growth. See also Kirzner

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of inputs, and try to avoid the inefficient use of resources. As entrepreneurs, they look for new and improved methods of production, and they look for ways to improve the characteristics of their output. For a firm to survive in the long run, good management is helpful, but successful entrepreneurship is essential. When one factors in the impact of economic progress on firms over time, it is apparent that a firm, no matter how efficiently managed, cannot survive by minimizing costs and eliminating waste. It must continually improve its methods of production, and it must continually update its product, or its business will be eroded by other firms that do. This is obvious when one looks at the difference between products in the computer and telecommunications industries now versus 10 years ago. Similar progress, though slower, is also apparent in automobiles, apparel, entertainment, and just about any industry. Even in an industry as basic as food, restaurants continually update their menus and preparation methods, and grocery stores continue to offer new products.

Conceivably, an innovative firm could continue to stay ahead of its competition despite some inefficiency, but an efficient firm will continue to lose ground in the marketplace if it does not innovate. Henry Ford, who implemented the brilliant innovation of using the assembly line to produce automobiles, and who was notorious for monitoring employees to prevent shirking and waste, once said his customers could have any color car they wanted, as long as it was black. But faced with eroding sales to General Motors, which offered a variety of colors, Ford changed his mind. This is one example of a more general observation that firms cannot hope to remain profitable by finding a successful formula and sticking to it. They will be left behind by competitors who discover improved production methods and improved product characteristics.

To emphasize again, for firms to succeed in a competitive market, efficient management is helpful, but entrepreneurship is essential. With this in mind, one must question an economic model of competition that assumes that competition is characterized by firms producing homogeneous products, and that depicts a competitive industry as one in which there are no profits. The first step to take to rectify this is to recognize the distinction between management and entrepreneurship; the second step is to orient the theory of competition to recognize the importance of entrepreneurship relative to management.

Profit and Loss

All attempts at entrepreneurship are not successful. People often believe they have spotted profit opportunities, and act on them, only to end up taking losses. Mises (1952, chap. 9), in a chapter with the same title as this section,

(1982; 1985, chap. 4) who makes the distinction between entrepreneurship and management, and emphasizes the importance of entrepreneurship to economic growth.

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emphasizes the role of profit and loss in directing resource allocation in an economy. Successful entrepreneurship results in profits–by definition, if one defines entrepreneurship as spotting and acting on a previously unrealized profit opportunity.[9] The profits of the entrepreneur encourage others to imitate the entrepreneur's actions, eventually competing those profits away. Losses signal that what at first appeared to be an entrepreneurial opportunity was, in fact, not profitable, so not actually an entrepreneurial opportunity. People who attempt entrepreneurial actions take risks, as Rothbard (1997, p. 246) emphasizes. Profits give positive reinforcement to successful entrepreneurship, while losses discourage the further pursuit of unsuccessful attempts at entrepreneurship. Market incentives lead entrepreneurial actions to generate economic progress, but limit the negative impacts of unsuccessful attempts at entrepreneurship.

Entrepreneurial opportunities are rarely as simple as arbitrage, and the complexities of production and time may turn what appears to be a profit opportunity into a losing activity. For example, Apple Computer introduced the first handheld computer, the Newton, in 1993, but it was not profitable. Despite the failure of the Newton, 3Com Corporation introduced the Palm Pilot handheld computer in 1996, and it was an immediate success. Similarly, Xerox developed the graphical user interface for computers, including the use of on-screen windows and the mouse as a pointing device, but failed to turn their invention into a commercially successful product. Apple commercialized the idea in their Macintosh computer. Apple failed to spot a true profit opportunity to produce handheld computers, and Xerox failed to spot a true profit opportunity to produce a windows-based computer operating system, not because those general ideas could not lead to profits, as later entrants showed, but because their originators were not able to put all the pieces together to make those ideas profitable.

This is where profits and losses guide entrepreneurial activity, with losses limiting ideas that are not wealth-enhancing and profits reinforcing ideas that are. The result is that the positive impact of successful entrepreneurship is much larger in magnitude than the negative impact of unsuccessful attempts. One can spot those successful entrepreneurial actions in hindsight, by the profits they generate, but it may be impossible to say ahead of time what someone could do to be entrepreneurial. As Baumol (1993, p. 15) notes, entrepreneurial activity is innovative, and if it can be completely described and explained, it can be reduced to a managerial activity. Thus, Baumol suggests,

[9]Mises (1973, pp. 289-94) allows for the possibility that entrepreneurs can take losses as a result of their entrepreneurial decisions, so entrepreneurial acts according to Mises are not always in response to profit opportunities. In this regard, Mises appears closer to Rothbard (1997) than Kirzner (1972) on the issue of what constitutes entrepreneurship.

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it may not be possible to completely explain how entrepreneurial actions take place.[10]

Circumstances of Time and Place

Hayek (1945) emphasized the particular circumstances of time and place that provide knowledge to some people that is unavailable to others. While Hayek was discussing the role of the market system in coordinating the activities of individuals with this type of specific knowledge, his observations are directly relevant to entrepreneurship. Individuals without such knowledge cannot simply rely on their alertness to spot profit opportunities, because information about a profit opportunity may not be sufficient without a context of knowledge which allows individuals to see what actions might result in profits, as Holcombe (forthcoming) notes. For example, it would be difficult for an auto mechanic to spot a profit opportunity in the pharmaceutical industry, and it would be difficult for a dentist to spot a profit opportunity in the automobile industry, even if they might wish for a product with certain currently unavailable characteristics. Some examples can help illustrate the distinction between entrepreneurship and management.

Desrochers (2001), in a very insightful article, offers many examples. One relates a story about a demonstration Steve Wozniak, a cofounder of Apple Computer, saw of a color monitor connected to a computer. Wozniak was so impressed that he decided the Apple II–their first successful commercial product–had to have a color monitor. Wozniak says that had he not seen the demonstration, Apple computers probably would have had monochrome monitors, and probably would not have been as successful. The color monitor differentiated his product from other personal computers at the time, which was the type of competitive action that (1) results in economic progress, and (2) would be called a deviation from competition by neoclassical economics, because of the product differentiation. Desrochers offers many other examples showing how specific knowledge of time and place leads to entrepreneurial activity.

Another example of entrepreneurship is offered by Collins (2002), who discusses changes in production methods for Piper aircraft. Typically, aircraft manufacturers have different assembly lines for different aircraft models. Collins observes,

> In the new plan, all the different models go down the line together–a turboprop Meridian following a Warrior, for example. . . . The first thought is that the Meridian would take a lot more time at each station, so it would slow the moving of the line. That doesn't happen because if the technicians finish on the Warrior and the Meridian isn't ready, they go to that station to help complete the task. . . . I've been looking at aircraft plants for years, and this is the

[10] In a later work, Baumol (2002) suggests that entrepreneurial actions may be responsible for only a small part of economic progress, with routinized research and development within corporate bureaucracies being responsible for most economic progress. The analysis here would take issue with Baumol's assessment.

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> best-looking system I have ever seen. New Piper estimates that it will result in a significant decrease in the man hours required to build an airplane. (Collins 2002, pp. 24-27)

The end product—the airplane—is the same, but a more efficient production model produces it, yielding an example of the shift from $f(K,L)$ to $g(K,L)$. A profit opportunity like this is not just something that an observant individual could notice. It requires specific knowledge of time and place—in this case, knowledge about aircraft manufacturing—for one to recognize that a different production process might be more efficient.

Adam Smith notes the same thing when he says,

> Men are much more likely to discover easier and readier methods of attaining any object, when the whole attention of their minds is directed towards that single object, than when it is dissipated among a great variety of things. . . . It is naturally to be expected, therefore, that some one or another of those who are employed in each particular branch of labour should soon find out easier and readier methods of performing their own particular work, whenever the nature of it admits of such improvement. (Smith 1776, p. 9)

Thus, Smith recognizes how the specific knowledge of time and place leads to entrepreneurial discovery, and why entrepreneurial discovery is necessary for a business to survive. Because competitors will be taking advantage of this type of knowledge to act entrepreneurially, the firm that continues to employ the same methods of production will be left further and further behind others in the industry with the passage of time.

Henry Ford's adoption of the assembly line to manufacture automobiles is an example from the beginning of the twentieth century of innovations in both the type of output, with the Model T differentiated from other automobiles, and in the process of manufacturing. Christensen (1997) gives several examples from the computer industry in the late twentieth century of the same type of innovation. He notes several examples where successful firms retained their successful formulas too long, only to be left behind by the progress of the market. For example, IBM focused on their mainframe computer business as their market was being eroded in the 1970s by companies like Digital Equipment Corporation (DEC) that produced lower-cost minicomputers. DEC in turn stuck with their successful minicomputer line in the 1980s as the market was shifting to personal computers. In both cases, the firms ran into financial troubles as progress in the market made their once-successful formulas increasingly less successful. Even experts in an industry, who see the information about the direction of the market, may find themselves unable to place it in the context of knowledge that shows them the entrepreneurial actions that must be taken for the firm to continue to be successful. When one looks at the march of economic progress, Schumpeterian entrepreneurship that disrupts the existing order is an integral part of the process.

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Is Entrepreneurship Equilibrating or Disequilibrating?

As Holcombe (1999) notes, whether one views entrepreneurship as equilibrating or disequilibrating depends in part on how one defines equilibrium. In Kirzner's (1973) view, an economy is in equilibrium only when there are no unexploited profit opportunities, which by definition requires an economy to be out of equilibrium for entrepreneurship to take place. Hayek (1949, p. 41) defines equilibrium differently, as the condition where "the different plans which . . . individuals . . . have made for action in time are mutually compatible." Similarly, Hahn (1984, p. 44) says equilibrium exists when "the intended actions of rational economic agents are mutually consistent and can, therefore, be implemented." Lewin (1997, p. 245) says "equilibrium is understood to be the consistency of actions and the plans on which they are based." One could envision a situation where everyone's plans are mutually consistent, so are in equilibrium according to the definitions of Hayek, Hahn, and Lewin, yet there are unrecognized profit opportunities so the Kirznerian criterion for equilibrium is not satisfied. This is more than just a terminological issue. Kirzner's description of entrepreneurship evokes the image of entrepreneurial actions leading toward an evenly rotating economy, to use the terminology of Mises (1966) and Rothbard (1962), where nothing changes period after period, whereas Schumpeter's description of entrepreneurship depicts actions that would disrupt the equilibrium of an evenly rotating economy. Kirzner's entrepreneur pulls an economy toward the existing equilibrium, while Schumpeter's (1934) entrepreneur creates a new equilibrium.

The new equilibrium created by Schumpeter's entrepreneurship is actually a preexisting equilibrium the way Kirzner defines the term, which partially resolves the question of whether entrepreneurship is equilibrating or disequilibrating. Kirzner (1992, p. 169) alludes to this as he differentiates two different types of Hayekian knowledge problems. The previously unnoticed profit opportunity implies, following Kirzner's use of the term, that the economy was not in equilibrium prior to the action of the Schumpeterian entrepreneur. Kirzner clearly shows that this is his view with an example:

> The truth surely is, we now see with 20-20 hindsight, that the horse-drawn carriage industry, for all its placid, normal-profitability over many decades, was an industry in grave disequlibrium *before* the automobile actually appeared. (Kirzner 2000, p. 250; italics in the original)

While one could look at it either way, depending upon how one defines a state of equilibrium, within the context of economic progress discussed above, the Schumpeterian view of the effect of entrepreneurship on markets better describes the long-run impact of entrepreneurship. In the short run, if the economy is not in equilibrium, entrepreneurship moves the economy toward equilibrium, as Kirzner describes. But in the long run, the creative destruction of entrepreneurship is what generates economic progress.

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Kirzner (1992) recognizes that the underlying economic data defining equilibrium are continually changing, so equilibrium is a moving target, but Kirzner notes that he focuses on entrepreneurial action with given underlying economic data and an unchanging underlying equilibrium. However, as Holcombe (1998) notes, one of the factors that causes the underlying equilibrium to change is the actions of entrepreneurs, so Kirzner focuses on one part of the entrepreneurial story but not the part that interested Schumpeter. This is not to say that one view of entrepreneurship is right and the other wrong, but to suggest that Kirzner focuses on the short-run equilibrating actions of entrepreneurs, whereas Schumpeter's sees the actions of entrepreneurs as disrupting the existing equilibrium and producing the changes that create economic progress.[11]

The Process of Entrepreneurial Discovery

Entrepreneurial discovery is depicted by Kirzner (1973) as the costless act of noticing a profit opportunity, in contrast to Romer (1990) who depicts technological advancement as the result of the intentional investment decisions of profit-maximizing firms. These two views lie at the extremes as models of entrepreneurial discovery. For Kirzner, the entrepreneur just notices a previously undiscovered profit opportunity, whereas Romer does not use the term entrepreneurship at all. Profit opportunities are produced, much like other intermediate goods in an economy. In fact, the process of entrepreneurial discovery is a combination of both of these extremes. There is more to entrepreneurship than just noticing that a profit opportunity exists, as Rothbard (1997) notes.[12] Entrepreneurs undertake many activities to search for entrepreneurial opportunities, and to make it more likely that they, rather than someone else, will be the discoverer. Still, undertaking research activities may produce entrepreneurial opportunities, but does not guarantee that they will be recognized.

An example that brings together the views of Romer and Kirzner is the innovation of the graphical user interface on computers. The concept was

[11]This issue has been discussed by a number of others, including Kirzner (2000, pp. 239-57). See, in particular, page 246, where Kirzner discusses the contributions of a number of authors who examine the differences between his and Schumpeter's views on entrepreneurship. Kirzner (2000, pp. 253-54) concludes, "The reconsideration here undertaken indeed permits us to see how both the Schumpeterian view of the entrepreneurial role and my own view can both be simultaneously accepted." Yet Kirzner notes that these views are not the same, and suggests insights within his view of entrepreneurship offers beyond Schumpeter's.

[12]Rothbard (1997) emphasizes the risk-bearing nature of entrepreneurial activity. This might be eliminated from the Kirznerian framework by arguing that someone who acts in order to try to seize a profit opportunity but takes a loss was not acting on a profit opportunity after all. To explore this line of reasoning—which is interesting in its own right—would lead away from the line of reasoning developed in this article.

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invented at the Xerox Corporation, where research and development activities had been undertaken. Xerox came up with the idea of using windows on the computer screen and the mouse as an input device to navigate the windows operating system, but failed to commercialize their invention. Steve Jobs at Apple Computer saw Xerox's invention as a profit opportunity and created the Apple Macintosh computer with the new concepts at its foundation, followed by Bill Gates who adapted the graphical user interface as Microsoft Windows. Schumpeter (1934) made the distinction between invention and innovation, and using Schumpeter's terminology, the invention was made at Xerox, but the innovators were Steve Jobs at Apple and Bill Gates at Microsoft.

Romer's view, where investment in research and development produces technological change, which then results in economic growth, leaves out the step where invention becomes innovation. In the graphical user interface example, Xerox invested in the research and development, but Xerox was unable to capitalize on the commercial potential of that invention, and without a market economy conducive to entrepreneurship, the story might have ended there. Jobs and Gates, though not the inventors of the concept, were the innovators, who recognized an unexploited profit opportunity and acted entrepreneurially to capture it in the manner described by Kirzner. But those entrepreneurs did not simply happen to notice what nobody before them had seen, they had systematically built up their bases of knowledge, as Holcombe (forthcoming) describes, and were using their knowledge of the computer industry as a base to actively seek profit opportunities from innovations in the computer market. They had invested resources in generating what Hayek (1945) called that specific knowledge of time and place, so that when the profit opportunity presented itself, they had both the alertness and the deliberately acquired stock of knowledge to recognize it.

Entrepreneurship is not produced, but the environment within which entrepreneurial discoveries are more likely can be produced, and entrepreneurs do invest in producing the knowledge base that will allow them to make future entrepreneurial discoveries. Harper (1996) discusses entrepreneurial activity within a growth-of-knowledge framework and recognizes that entrepreneurs do create an environment within which entrepreneurial discoveries are more likely.

When analyzing the entrepreneurial process, the actions of entrepreneurs can be broken down into three components along the lines of the example above. First, entrepreneurs can undertake activities like research and development that may generate profit opportunities. Second, entrepreneurs can build their stock of knowledge so that when a profit opportunity does appear, they will be in a position to recognize it. Third, there is that elusive characteristic of alertness that enables an entrepreneur to spot what others have not recognized.

There is a second aspect to the entrepreneurial process that goes beyond the actions of the entrepreneur and extends to the entrepreneur's economic

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environment. Desrochers (2001) discusses the way that knowledge can be tacitly shared and obtained by those who are in close geographic proximity, increasing the overall level of entrepreneurial activity. Holcombe (1998) notes that entrepreneurial actions on the part of some generate entrepreneurial opportunities for others. Some economic environments are more conducive to entrepreneurship than others. These environmental factors might also be broken down into three components. First, there are those aspects of the economic environment that produce profit opportunities; second, there are those aspects of the environment that make it easier to recognize those profit opportunities that exist; and third, there are those aspects of the environment that create the incentive for entrepreneurs to act on those profit opportunities they spot. A complete understanding of the entrepreneurial process requires a recognition of both the way in which the economic environment generates entrepreneurial opportunities and invites entrepreneurs to take advantage of them, and the way that entrepreneurs themselves act to increase the chances that they will make an entrepreneurial discovery.

Competition and Entrepreneurship

Kirzner's (1973) book with the same title as this section offers a theory of the competitive process as an alternative to the neoclassical theory of competition which depicts competition as an equilibrium outcome. The neoclassical model of competition depicts an economy already at equilibrium, with no profit opportunities for firms. The only role for firms is efficient management. If a competitive market is not at an equilibrium, how would it get there? Kirzner answers that entrepreneurs notice unexploited profit opportunities that firms can act on, leading markets to equilibrium. Entrepreneurs serve this equilibrating function in response to any factors that might disturb a market equilibrium. Yet, more along the lines of Schumpeter, entrepreneurs must also be innovators, and must find new and improved types of output, and new and improved ways of producing their output, to survive.

The competitive process is the force that propels economic progress, and because the competitive process generates progress, firms must be entrepreneurial to survive. Firms cannot simply minimize costs and hope to remain profitable in a market where their competitors are offering innovations in output, and are discovering innovative methods of production. Firms must continually innovate to remain profitable. Entrepreneurship might be thought of as arbitrage, where the alert entrepreneur notices that a good can be bought for less in one location than it can be sold for in another, but this greatly oversimplifies the entrepreneurial role. Two complicating factors are production and time. It may be that the arbitrage opportunity involves buying inputs and combining them to produce output in a way that the cost of the inputs is less than the sales price of the output, but this is considerably more complex than simple arbitrage. Also, while it may be that if the inputs were bought today

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and the output were sold today, the inputs would cost less than the price of the output, that is no guarantee that in a production process that takes time, the inputs bought today could produce output that could sell for more than enough to cover their cost tomorrow.

In most cases, entrepreneurship is much more complex than arbitrage. It is more than just being alert to price discrepancies in the market. It is spotting alternative methods of production, and spotting ways in which output characteristics can be altered to better satisfy the demands of purchasers. Those entrepreneurial activities will typically require what Hayek (1945) called the specific knowledge of time and place. Simply being alert is not sufficient. One must have the appropriate context of knowledge within which to place new information in order to recognize that a profit opportunity exists.[13] Because an entrepreneurial opportunity has not been previously acted upon, there is always some uncertainty involved in an entrepreneurial undertaking, to use Knight's (1921) term, so without some reward for entrepreneurship, there would be no reason to accept the uncertainty. There is a tendency to look at successful examples of entrepreneurship and overlook the failures, but entrepreneurial actions almost always entail the likelihood of failure. Entrepreneurial action does not guarantee profits, but in a competitive market, a lack of entrepreneurship assures that profits will be eroded over time until they disappear.

The competitive process can be described briefly as follows. At any point in time, the economy tends toward an equilibrium, and if it is not exactly at that equilibrium, entrepreneurial actions, as described by Kirzner, pull the economy toward equilibrium. That equilibrium is always changing, partly because of changes in preferences, resource prices, and other factors exogenous to the producers in the market. When these factors exogenous to the firms in the market change, Kirznerian entrepreneurship is the equilibrating force, as Kirzner (1992, pp. 39-42) describes. A market's equilibrium will also change because of the entrepreneurial actions of the firms in the market. The process of competition implies innovations in production that lower production costs, and innovations in output characteristics, so competitive firms must always be alert to these innovations and must match them or improve on them to remain in business. Entrepreneurial innovations are always upsetting the previous equilibrium and creating a new one, and each new equilibrium offers consumers lower costs and/or more desirable products. What appears in neoclassical theory as an unchanging equilibrium is in fact a moving target. The neoclassical competitive outcome, where firms produce a homogeneous product according to a given production function, is completely unrelated to the competitive process where firms differentiate their products to enhance their appeal to consumers, and where they change their production processes to lower their costs.

[13]Holcombe (forthcoming) discusses this aspect of entrepreneurship in more detail.

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Competitive firms cannot survive over time without being entrepreneurial. If they simply act as the neoclassical theory describes, producing according to a given production function, choosing the optimal levels of inputs, and minimizing input waste (such as shirking employees), they will fall further and further behind as the market adopts new production processes and offers new output characteristics. Entrepreneurship is an integral part of the competitive process, and an essential ingredient in any competitive firm, even though it is not represented in the neoclassical theory of the firm. The competitive process is driven by entrepreneurship that produces continually better products–as judged by the consumers of those products–and that produces those products at costs that continue to fall over time because of innovation in production techniques. In this way, entrepreneurship produces economic progress.

Three Differences

To highlight the significance of this entrepreneurial approach to competition and economic progress, this section considers three aspects that differentiate it from the equilibrium approach to competition and growth that dominated twentieth century economic analysis. Product differentiation is viewed differently in the two approaches, the implications of profit are different, and most significantly, the entrepreneurial approach offers a substantially different view of optimality. There are other differences, but by focusing on these three significant differences, one can see that this approach offers not just a critique of existing theory, but an alternative way of viewing competition and economic progress.

Product Differentiation

The issue of product differentiation really goes back to the distinction between growth and progress. If one proxies progress by growth, as in the standard view, and sees improvement as represented by higher income levels, then product differentiation is a means by which consumers can have more variety. Because differentiated products give sellers some monopoly power and allow them to price above minimum average cost, differentiated products result in an inefficiently large number of producers each producing a sub-optimal level of output, resulting in inefficiency, according to the standard view.[14]

[14]While there are many complex models of product differentiation, all of the standard models depict product differentiation as the offering of a greater menu of choices, and standard theory continues to depict the cost of this variety as the degree of monopoly power that comes with differentiated products. For a recent overview, see Church and Ware (2000, chap. 11), which is devoted to giving a survey of the product differentiation literature. Shapiro and Varian (1998, chap. 3) provide an interesting analysis of product differentiation in an information economy, where firms have an incentive to provide different products customized to the demands of different customers; for example, different versions of a software program tailored to different markets. Again, however, the model is one of product variety rather than improvements in quality that generate economic

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There is a trade-off between benefits of greater variety and the costs of some monopoly power that goes with differentiated products, and whether product differentiation is desirable according to the neoclassical approach depends upon how one evaluates this trade-off.

In contrast, when one sees progress as more than just income growth, product differentiation is a competitive strategy that generates progress. One of the most significant ways in which progress manifests itself is through improvements in the types of output offered in the market, in the form of either new products or improved characteristics in existing products. Improvements in output occur when entrepreneurs see that it would be more profitable to change a product's characteristics, or offer a different type of product on the market. Product differentiation is the route through which innovation expresses itself, and through which progress occurs. The process of product differentiation is the process of improving the characteristics of economic output. Without firms that are continuing to differentiate their products, there would be no improvement in the types of output the market offers.

Cox and Alm (1998) show the substantial increase in product differentiation over only a few decades at the end of the twentieth century. In the early 1970s, they note (p. 4) that there were 140 different types of vehicle models consumers could buy, and 654 different styles of vehicles. By the late 1990s, there were 260 different vehicle types and 1,212 different styles. There were 140 different types of breakfast cereals in the early 1970s, and 340 different types in the late 1990s, and over the same time period the number of nationally distributed soft drink brands rose from 20 to 87, the types of milk offered to consumers rose from four to 19, the styles of running shoes increased from five to 285, and the types of bicycles rose from eight to 31. They offer many more examples of increasing product differentiation to show how the market enhances the availability of consumer choices by offering what they call mass customization. They emphasize the advantages in terms of a greater variety of consumer choices, but as beneficial as the increased number of choices is, the real long-run advantage of this product differentiation is that it introduces improvements in products available to people, yielding economic progress.

Product differentiation is not just an outcome that gives consumers more choices, it is a process that creates improved options for purchasers and generates economic progress. If one takes a static equilibrium view of the economy, product differentiation at any point in time appears to be the variety of choices that is offered to purchasers.[15] When one puts product differentiation within the context of economic progress, new variants of products improve

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progress. In this case, product differentiation, while it still confers monopoly power, allows price discrimination and so reduces the monopoly welfare losses to a degree.

[15]This in itself is desirable, because it allows people to better find goods and services that satisfy their own particular desires rather than having to accept a product that is aimed at the middle of the market.

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upon previous offerings, leading to economic progress. Product differentiation is an essential component of a dynamic economy that increases people's well-being by offering them increasingly better products.

Profit

Another difference between the managerial and entrepreneurial views of the firm's activities is in the role of profit. In the managerial model, there is no profit in competitive markets because all profit is competed to zero. Because the competitive equilibrium is the benchmark for efficiency, profit is a sign of inefficiency and an impediment to welfare maximization.[16] The general equilibrium approach to economics depicts the concept of equilibrium as more than just a state toward which the economy is tending, and represents an economy as always in equilibrium, which means always without profit.[17] But profit is the lure which entices entrepreneurs to innovate. As Schumpeter (1934) notes, without profit there will be no entrepreneurship, and without entrepreneurship, there will be no profit. Do profits enhance economic welfare, or degrade it? From the managerial perspective, profits lower welfare because they raise prices, lowering the quantity demanded, keeping the economy from achieving Pareto optimality. Monopolies are the extreme case.[18] In competitive markets, profits are a sign of disequilibrium which, again, lowers welfare. From an entrepreneurial perspective, profits are the reward for welfare-enhancing innovation. They reward the entrepreneur for improving resource allocation, and create an incentive for further innovations to generate economic progress, so profits enhance economic welfare, and are necessary for economic progress.

Optimality

In the neoclassical framework, optimality means allocating resources such that nobody could be made better off without making someone else worse off. This is the economist's definition of Pareto optimality, which is almost always the concept of optimality that economists use. Pareto optimality is a static concept that judges the way that resources are allocated at any one point in time, but a static criterion for optimality is inappropriate for evaluating the characteristics of an economy that is continually improving its performance over time. Some characteristics of an economy prevent it from attaining Pareto

[16]The standard theory does allow for a "normal profit," which is just sufficient to keep the business from leaving the market, but this is a return to a factor of production, not profit as the term is ordinarily used. See Bator (1957) and Graaf (1957) for expositions of the competitive outcome as a benchmark for efficiency.

[17]This assumes a competitive general equilibrium. It is possible that some firms would have monopoly power in equilibrium, allowing for monopoly profit, but this profit would not serve the same role as profits in a market process view, because the profits would remain permanently as a part of the equilibrium condition.

[18]For a critique of government policy toward monopoly, which is based on an equilibrium approach to markets, see Armentano (1972, 1982, 1986).

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optimality, but enhance economic welfare, so a Pareto optimal allocation of resources is not the welfare-maximizing allocation. Two characteristics that fit this description are product differentiation and profit, as just described. Competitive markets allocate resources Pareto optimally when they offer consumers homogeneous products, and when firms are making no profits, but welfare would be reduced in an economy that did not have these non-Pareto optimal characteristics. For economic progress to occur, which improves people's welfare, both profit and product differentiation are necessary. Profit guides entrepreneurs to better satisfy people's wants, and product differentiation is the vehicle that improves the characteristics of the economy's output. An economy that satisfies the static criterion of Pareto optimality is therefore not maximizing people's welfare.

The neoclassical framework, as described by Graaf (1957) and Bator (1957), depicts welfare maximization as the Pareto optimal allocation of resources, and twentieth-century growth theory, built on the foundation of Solow (1956) extends this framework to a dynamic setting so that there is a Pareto optimal growth path. That framework is mathematically elegant and logically correct, but it is flawed because it represents economic progress as growth in a homogeneous measure of output.[19] Because changes in the characteristics of output are more important components of economic progress than income growth, the concept of optimality must be modified, and a Pareto optimal allocation of resources eliminates some of the features of an economy that generates economic progress. If resources are allocated Pareto optimally, the conditions of Pareto optimality take away the incentive to act entrepreneurially.[20] The concept applies to a static steady-state economy, but not to one in which welfare improvements are generated by economic progress.[21]

If welfare maximization means making people as well-off as possible, economic progress is crucial to welfare maximization, and aiming for the goal of a static Pareto optimal allocation of resources hinders welfare maximization.[22]

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[19]Holcombe (1989) notes that all models are built on simplifying assumptions, and that simplifying assumptions are a virtue because they enable people to see the essential features of a process by assuming away irrelevant or unimportant features. However, there is a danger that some things may be assumed away that are essential for understanding the issue at hand, in which case the model will be misleading. This is the case with growth theory at the beginning of the twenty-first century.

[20]Pareto optimality could be redefined so that it means that nobody's lifetime welfare could be increased without reducing someone else's, but this would really be a different concept. To see this, consider the conclusion that monopolies allocate resources sub-optimally as Pareto optimality is applied. If monopoly profits are the result of entrepreneurial activity, those profits are the reward that produces progress, not a deadweight loss as they are represented to be in the Paretian framework.

[21]Rothbard (1956) offers a different and more general critique of neoclassical welfare economics.

[22]Examples from a public-policy standpoint include antitrust policy, laws that dictate product standards and specifications, and product quality laws such as those set by the

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Welfare maximization is a process, not an outcome, and the process is driven by entrepreneurship, and manifests itself as economic progress.

Conclusion

This article presents a description of the competitive process that is based on entrepreneurship, and that produces economic progress. The framework described here differs substantially from the neoclassical competitive framework for two main reasons. First, the neoclassical framework assumes away any role for entrepreneurship, so the people who run firms in that theory are managers, not entrepreneurs. Second, in its dynamic form, the neoclassical framework focuses on growth in the quantity of output, rather than on progress, which consists primarily of changes in the types of output produced. The difference is progress versus growth, and entrepreneurship versus management. One of the key questions in economics, described well by the title of Adam Smith's *The Wealth of Nations* (1776), is how nations become prosperous. The ideas presented here suggest that twentieth-century equilibrium economics missed two key aspects that underlie prosperity. By focusing on income growth rather than progress in the types of goods and production methods, economics has analyzed the wrong indicator of prosperity. By focusing on efficiency in production rather than innovation, economics missed the key factor that generates economic progress.

Progress can be made in economics just as it can in the market economy, and this article suggests that progress in the theory of economic growth can best be made by recognizing that the scope of economic progress is much broader than income growth, and that entrepreneurship is the activity that generates economic progress. This article describes how theories of entrepreneurship can be completely incorporated into a model of the competitive process to show that entrepreneurship is the engine of economic progress, to show that entrepreneurship is necessary for firms to survive in competitive markets, and to show that product differentiation is one of the competitive strategies that produces economic progress. The result is not a steady-state competitive equilibrium, nor would such a result be optimal. The result is a competitive process where firms continually improve their production processes and the characteristics of their output in order to remain competitive, and this process results in continual economic progress.

Food and Drug Administration. All of these types of regulations are designed to make the allocation of resources more closely approximate a Pareto optimal allocation.

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DIMENSIONS[1] AND ECONOMICS: SOME PROBLEMS

WILLIAM BARNETT II

> . . . the units of all physical quantities, as well as their magnitudes, [should be included] in all of his calculations.[2] This will be done consistently in the numerical examples throughout the book.
>
> –Sears and Zemansky
> *University Physics* 1955

The consistent and correct use of dimensions is essential to scientific work involving mathematics. Their very existence creates the potential for errors: omitting them when they should be included, misusing them when they are included, and others. However, their existence also makes possible dimensional analysis, which can be a significant factor in avoiding error. In the equation $y = f(\cdot)$, if y should have dimensions then so also should

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[1]Throughout, "dimensions" is used generically and "units," specifically. Thus, distance is a dimension and centimeters, meters, and feet are among the alternative units of the distance dimension.

[2]Sears and Zemansky (1955, p. 3) distinguish units and magnitudes, magnitudes being pure numbers, as follows:

> We shall adopt the convention that an algebraic symbol representing a physical quantity, such as F, p, or v, stands for both a *number* and a *unit*. For example, F might represent a force of 10 lb, p a pressure of 15 lb/ft^2, and v a velocity of 15 ft/sec. When we write $x = v_0t + \frac{1}{2}at^2$, if x is in feet then the terms v_0t and $\frac{1}{2}at^2$ must be in feet also. Suppose t is in seconds. Then the units of v_0 must be ft/sec and those of a must be ft/sec2. (The factor $\frac{1}{2}$ is a *pure number*, without units.)

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f, and they should be identical to those of y. If y should not have them then neither should f have them. Such an analysis of $y = f(\cdot)$ would determine: (1) which, if any, dimensions y and each element of f, and consequently f, itself, should have; and, (2) whether the dimensions of f and y are identical, which is a necessary, though not sufficient, condition for the equation to be correct. An error revealed by a correctly performed dimensional analysis indicates a fundamental problem.[3] Therefore, the importance of dimensions for science can hardly be overstated.

The first sections of this paper consider, respectively, the following two problems that arise when dimensions are not correctly included in economic models: (1) those that are meaningless or economically unreasonable; and, (2) those that are inconstant–i.e., the same constant or variable having different dimensions, as if velocity were sometimes measured in meters per second and other times measured in meters only or in meters squared per second.[4] The third section provides a macroeconomic example of the "dimensions problem" from an article in a recent issue of a leading English language economics journal. Section four contains a discussion; and the final section, the conclusions.

The analysis in this paper concerns production functions and is robust with respect to increases in the number of independent variables and to alternative functional forms.[5] Moreover, the analysis is robust with respect to others used in economic theory: e.g., utility, demand, and supply functions.

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[3]Sears, Zemansky, and Young state:

> When a problem requires calculations using numbers with units, the numbers should always be written with the correct units, and the units should be carried through the calculation as in the example above. This provides a useful check for calculations. *If at some stage in the calculation you find an equation or expression has inconsistent units, you know you have made an error somewhere.* In this book we will always carry units through all calculations, and we strongly urge you to follow this practice when you solve problems. (1987, p. 7; emphasis added)

"Dimensional analysis is used to check mathematical relations for consistency of their dimensions . . . *[i]f the dimensions are not the same, the relation is incorrect.*" (Cutnell and Johnson 2001, p. 6; emphasis added)

[4]These are *not* those of aggregation in disguise; they can, and do, exist in models of but one good and one resource, labor. Rather, the issues dealt with here are even more basic and devastating for mathematical economics and econometrics than is that of aggregation.

[5]Although not directly related to the subject under discussion, it should be noted that there is a fundamental problem with the use of the mathematics of functions in economics. One *sine qua non* of a function is that, for any specific set of values of the independent variables, there must be a *unique* value of the dependent variable.

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MEANINGLESS OR ECONOMICALLY UNREASONABLE DIMENSIONS

One widely used function[6] is a 2-input "Cobb-Douglas" (CD) production function. A typical CD function is given by $Q = AK^{\alpha}L^{\beta}$, in which: Q is the output variable; K and L are the capital and labor input variables, respectively; A, may be a constant or a variable; and, α and β are the elasticity of output with respect to capital and with respect to labor, respectively. Consider a 2-input, CD, production function for a specific good, widgets: $Q = AK^{\alpha}L^{\beta}$. If dimensions are used correctly, output, capital, and labor each must have both magnitude *and dimension(s)*, while α and β are pure numbers. Assume, for example,that:[7]

(1) Q is measured in *widgets/elapsed time (wid/yr)*;
(2) K is measured in units of *machine-hours/elapsed time (caphr/yr)*; and,
(3) L is measured in *man-hours/elapsed time (manhr/yr)*.

Then a dimensional analysis of the production function $Q = AK^{\alpha}L^{\beta}$ establishes that A $(= Q/K^{\alpha}L^{\beta})$ is measured in $(widgets/elapsed\ time)/([machine\text{-}hours/elapsedtime]^{\alpha} \cdot [man\text{-}hours/elapsed\ time]^{\beta})$; i.e., in $(wid \cdot yr^{(\alpha+\beta-1)})/(caphr^{\alpha} \cdot manhr^{\beta})$.

Only positive values of α and of β are acceptable, as nonpositive values for either, or for both, imply negative or zero marginal productivity of the relevant input(s). If $\alpha = \beta = 1$, then the dimensions of K^{α}, L^{β}, and Q–machine-hours

"Let X and Y be nonempty sets. Let f be a collection of ordered pairs (x, y) with $x \in X$ and $y \in Y$. Then f is a function from X to Y if to every $x \in X$ there is assigned a unique $y \in Y$" (Thomas 1968, p. 13; emphasis added).

Therefore, it is incorrect to express production relationships in any case in which Leibensteinian style *X*-inefficiency can exist. For an example of such a situation see note 11.

[6]This is an understatement. It is probably no exaggeration to claim that the CD is the most widely used mathematical example in all of neoclassical economics.

[7]Because there are no standard systems of dimensions or units in economics, specific, but nonstandard, units are used. It should be noted that, so long as matters are confined to mathematical models, the issue of dimensions/units can simply, though indefensibly, be ignored, this is no longer true when the matter turns to the estimation of econometric models. Then, data must be used. If every variable is measured in monetary terms, the problem of dimensions does not arise. Of course, measuring every variable in monetary terms raises other problems. For example, although some input variables may be, and sometimes are, measured in terms of nonvalue (i.e., "real") units (e.g., of man-hours for labor input), the input of capital goods is invariably measured in value (i.e., monetary) units, and the output is virtually always measured in monetary units. On the one hand, this raises the aggregation problem *re* heterogeneous capital goods; on the other it presents the difficulty of the circularity of the measurement of the value of the capital because of the role of the interest rate in determining the present value of a quantity of capital goods and the role of the quantity of capital goods in determining the interest rate. On these points see Harcourt (1972, pp.1-46).

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per year, man-hours per year, and widgets per year, respectively–are meaningful. But, the dimensions of A are widgets per (machine-hours • man-hours) per year or wid/(caphr•manhr)/yr. For those dimensions to be meaningful, requires, at a minimum, that the product of machine-hours and man-hours is meaningful, a dubious proposition indeed. However, even if the dimensions are meaningful in this case, they are economically unreasonable. For, if $\alpha = \beta = 1$, the marginal products of both K and L are positive constants (the Law of Diminishing Returns is violated) and there are unreasonably large economies of scale–a doubling of both inputs, *ceteris paribus*, would quadruple output.

Alternatively, if it is not true that $\alpha = \beta = 1$, then either α or β, or both, have noninteger values or integer values of two or greater. Noninteger values of α or β, or both, result in such units as, for example, $(\text{man-hours/year})^{0.5}$ or $(\text{man-hours/year})^{1.5}$ for L^{β}, and similarly for K^{α}. But the square roots of man-hours and of years are meaningless concepts, as are the square roots of the cube of man-hours and the cube of years. Also, integer values of two or greater for α or β, or both, result in such units as, for example, $(\text{man-hours/year})^2$ or $(\text{man-hours/year})^3$ for L^{β}, and similarly for K^{α}. But the squares of man-hours and of years are meaningless concepts, as are the cubes of man-hours and of years, and similarly for machine-hours. (The units of A are even more meaningless, if that is possible.) Therefore, no matter what the values of α and β, the dimensions are either meaningless or economically unreasonable.

If the same 2-input, CD, production function, $Q = AK^{\alpha}L^{\beta}$, is used, but Q is taken to be aggregate output, then the function is an aggregate, or macroeconomic, production function. However, and for the same reasons as in the microeconomic example, a correct use of dimensions here also yields dimensions that are either meaningless or economically unreasonable. Moreover, an additional problem, that of aggregation, arises in the macroeconomic case.

The problem of dimensions that are either meaningless or economically unreasonable cannot be eliminated by using more complex production functions such as the constant elasticity of substitution (CES); if anything, it is exacerbated.

A correct use of dimensions in these examples, then, yields results that are either meaningless or economically unreasonable. However, these problems only become evident when dimensions are correctly included in the model, which is rarely[8] the case with economic modeling.

Inconstant Dimensions

To reiterate, this problem consists in the same constant or variable having different dimensions, as if velocity were sometimes measured in meters per second

Moreover, if real units are used, then production functions are consistent with economic theory in that particular quantities of the various inputs combine to produce a specific quantity of the output. However, if monetary units are used such production functions set economic theory on its head, for then particular values of the various inputs combine to produce a specific value of the output. But, economic theory teaches that the

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and other times measured in meters only or in meters squared per second. It can be illustrated by comparing (Newtonian) gravity[9] with production functions. Gravitation is a force. A force *(F)* exerted on a body may be measured as the product of its mass (m) times its acceleration (a);[10] i.e., $F = m \cdot a$. In the meter-kilogram-second (mks) system the units of F are kilograms • meters /(second^2); i.e., kg • m/(sec^2). Sir Isaac Newton's law of universal gravitation, may be stated:

> Every particle of matter in the universe attracts every other particle with a force which is directly proportional to the product of the masses of the particles [m and m'] and inversely proportional to the square of the distances between them [r^2].
>
> $$F \propto mm'/r^2.$$
>
> The proportion above may be converted to an equation on multiplication by a constant G which is called the gravitational constant:
>
> $$F = G \cdot (mm'/r^2).$$ (Sears and Zemansky 1955, p. 79)

Then: $G = F/(mm'/r^2)$. And, logical and physical consistency require that G have the dimensions of $F/(mm'/r^2)$. Using the mks system $F/(mm'/r^2)$ has the units (m^3)/(kg • sec^2); therefore, G must have the units (m^3)/(kg • sec^2).

This result has been invariant for countless measurements of G over the past three centuries: regardless of the magnitude, the dimensions have always been distance^3/mass • (elapsed time)^2; e.g., m^3/(kg • sec^2) in the mks system.

Unfortunately, such is not the case in economics. Compare that result–the constancy of the dimensions–with the results of measurements of a 2-input, CD production function. Such measurements yield estimates for α, β and A. Invariably, alternative estimates of α, β, and A differ. This is not surprising, but it does present a serious problem. Because A has both magnitude and dimensions, different values of α and β imply different dimensions for A, such that, even though the dimensions in which Q, K, and L are measured are constant, the dimensions of A are inconstant. For example, let Q, K, and L be measured in the same units as in the section "Meaningless or Economically Unreasonable Dimensions." Then, if the magnitudes of α and β are measured as 0.5 and 0.5, respectively, then the units of A are wid/(manhr$^{0.5}$ • caphr$^{0.5}$). However, if the magnitudes of α and β are measured as 0.75 and 0.75, respectively, then the units of A are (wid • yr$^{0.5}$)/(manhr$^{0.75}$ • caphr$^{0.75}$).

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value of the inputs is derived from the value of the output. (Thanks to an anonymous referee calling attention to this omission in the prior submission.)

[8]The only cases the author is aware of are a few instances involving Fisher's equation of exchange.

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The problem of inconstant dimensions (or economically unreasonable results) cannot be eliminated by using more complex production functions such as the CES; if anything, it is exacerbated.

A correct use of dimensions in this example, then, yields inconstant dimensions. Inconstant dimensions are, of course, a nonsensical result. However, this problem only becomes evident when dimensions are correctly included in the model, which is rarely the case with economic modeling.

Macroeconomic Example

Consider the following, from a model in a recent issue of a leading English-language economics journal.

1. In the section on households, the "[f]unction H measures the disutility from work, which depends on hours (N) and effort (U)." The arguments in the utility function of the representative household include $\Sigma\beta^t H(N_t, U_t)$; $t = 0 \ldots \infty$, where "$\beta \in [0, 1]$ is the discount factor" and t is the index of the time period.[11]

2. The section on firms posits that,

> [t]here is a continuum of firms distributed equally on the unit interval. Each firm is indexed by $i \in [0, 1]$ and produces a differentiated good with a technology $Yit = ZtLit^{\alpha}$. Li may be interpreted as the quantity of effective labor input used by the firm, which is a function of hours and effort: $Lit = Nit^{\theta}Uit^{1-\theta}$ where $q \in [0, 1]$. Z is an aggregate technology index, whose growth rate is assumed to follow an independently and identically distributed (i.i.d.) process $\{\eta_t\}$, with $\eta_t \sim N(o, s_z^2)$. Formally, $Z_t = Z_{t-1} \exp(\eta_t)$.[12]

3. The section on equilibrium maintains that,

> [i]n a symmetric equilibrium all firms will set the same price P_t and choose identical output, hours, and effort levels Y_t, N_t, U_t. Goods market clearing requires . . . $Y_{it} = Y_t$, for all $i \in [0, 1]$, and all t.

Furthermore, the model yields

> the following reduced-form equilibrium relationship between output and employment: $Y_t = AZ_tN_t^{\varphi}$.

---

[9]Although in this paper the analysis involves Newtonian gravity, the results of the analysis are robust for all applications in the natural sciences.

[10]It is true that a force is a vector quantity; i.e., it has a directional quality, as well as a magnitude (Sears, Zemansky, and Young 1987, p. 10). However, this is irrelevant for this analysis.

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Among the conclusions that can be drawn from this model, each of which will be considered in turn, are: (1) the number of firms and the number of households is identical, and is equal to infinity; (2) the quantity of each input used by each firm is identical to the quantity of each input provided by each household; and, (3) there are an infinite number of *differentiated* goods, each of which is *identical* to every other good.

First, the continuum of firms necessarily means that there is an infinite number thereof.[13] Assume, *arguendo*, that the (infinite) number of firms is given by n. Then, as each firm uses the same number of hours, N_t, and the same effort level, U_t, as every other firm, the total hours used are nN_t and the total effort level is nU_t. However, because N_t and U_t also are the hours and effort level of the representative household, unless there are exactly n households providing nN_t total hours and nU_t total level of effort, either the firms are using more hours than the households are actually working, or they are using less. The same can be said for the level of effort. Only if the number of households is n are the number of hours used and the level of effort used exactly equal to the number of hours worked and the level of effort provided. Of course, this would necessarily mean that there is an infinite number, n, of households exactly equal to the infinite number, n, of firms.

Second, because there would be one (identical but for the nature of the output) firm per (identical) household, each firm would use exactly the hours and effort level put forth by one of the households, though, conceivably, the hours and level of effort used by a particular firm would not all come from the same household.

Third, because $Y_t = AZ_tN_t^{\varphi}$,[14] and A and Z_t are both dimensionless magnitudes,[15] Y_t must have the same dimensions as N_t^{φ}. The dimension of N_t is

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[11]Obviously, the time period index, t, was inadvertently omitted from the consumption variable in the representative household's utility function.

[12]Because effective labor, L_{it}, is an argument in the production function of output, Y_{it}, and because effective labor is a function of the level of effort, U_{it}, Leibensteinian style X-inefficiency can exist in this model. On the importance of this, see note 5.

[13]"A set forms a continuum if it is infinite and everywhere continuous, as the set of reals or the set of points on a line interval" (Glenn and Littler 1984, p. 37).

[14]It could be argued that, because Y_t is the output of a single firm, $Y_t = AZ_tN_t^{\varphi}$ is not an example of a macroeconomic production function. However, because there are n identical (but for their differentiated goods) firms, the aggregate production function is $nY_t = nAZ_tN_t$. The microeconomic and macroeconomic functions, then are identical up to a linear scaling factor, n. That the firms' goods are differentiated does not prevent us from aggregating them in this model because, as is shown in the text, the differentiated goods are not differentiated at all; rather, they are identical. (If dimensions were being used, n and Y_t would have dimensions; e.g. firms, and widgets per firm in time period T, respectively. In that case, nY_t and Y_t would have different dimensions: widgets in time period T, and widgets per firm in time period T, respectively. However, because each firm's output

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hours (hrs); and φ is a positive, dimensionless, constant.[16] Therefore, the dimensions of Y_t are hrs$^{\varphi}$. In any case in which $\varphi \neq 1$, the dimension of Y_t, $(\text{hrs})^{\varphi \neq 1}$ is meaningless; e.g., $(\text{hrs})^{0.5}$, $(\text{hrs})^{1.5}$, and $(\text{hrs})^2$ are meaningless dimensions.[17] Alternatively, if $\varphi = 1$, then $Y_t = AZ_tN_t$, and the dimension of Y_t is the same as that of N_t, hrs. However, in that case, because $Y_t = AZ_tN_t$, the output hours are less than, equal to, or greater than, the input hours as AZ_t is less than, equal to, or greater than one (1). But if output is measured in hours, then the output hours cannot be greater than or less than the input hours; rather, the output hours must be equal to the input hours; i.e., $AZ_t \equiv 1$ and $Y_t \equiv N_t$. Therefore, each and every firm uses input of exactly N_t hrs to produce exactly N_t hrs of output; i.e., there is no net production–not one of the infinite number of supposedly profit maximizing firms produces more hours of output than the number of hours it uses as input.

Moreover, because $Y_{it} = Y_t \; \forall \; i$, the dimension of every firm's output is hrs. Therefore, each of the n differentiated goods produced by the n firms consists of homogeneous hours.

Surely, this model is not defensible.

DISCUSSION

The problems caused by the failure to use dimensions consistently and correctly in production functions–dimensions that are either meaningless, unreasonable, or inconstant–are not minor problems, and by no means are restricted to production functions. Rather, these problems are both critical and ubiquitous–they afflict virtually all mathematical and econometric models of economic activity. And that, unfortunately, is the way modern economics is done (Leoni and Frola 1977; and Mises 1977).

A more or less standard pattern can be discerned in articles in mainstream economics journals. First, the gist of a theory is concisely developed. Second, a more or less complex mathematical model of the theory is elaborated and solved. Third, an econometric model based thereon is constructed, and estimates of the magnitudes of the parameters and of the relevant statistics are provided. Fourth, there is an explanation and discussion of the empirical results. Fifth, conclusions are drawn. Sometimes some of the mathematical

is identical to every other firms' output, we could still validly aggregate their outputs by multiplying Y_t by n.)

[15]We are given that: $A \equiv [\lambda_n(1-\theta)/\lambda_u\theta]^{\alpha(1-\theta)/(1+\sigma u)}$; $\theta \in (0, 1)$ and is, therefore, dimensionless; and, λ_n, λ_u, σ_n, and σ_u are positive constants. We know that λ_n, λ_u, σ_n, and σ_u are dimensionless from the context in which they first appear: $H(N_t, U_t) = (\lambda_n N_t^{1+\sigma n}/(1+\sigma_n)) + (\lambda_u N_t^{1+\sigma u}/(1+\sigma_u))$. And, we know that α is a positive, dimensionless, constant from the context in which it first appears: $Y_{it} = Z_{it}L_{it}^{\alpha}$. Therefore A must be a positive dimensionless constant. We also know that Z_t must be a positive, dimensionless, variable

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manipulations may be relegated to an appendix if they are considered too abstruse for the body of the paper.

This methodology entails generating hypotheses or retrospective predictions, based on the theory, about the magnitudes of the relevant parameters of the model. Then, using the techniques of statistical inference, the estimated signs and magnitudes of the parameters are compared with their expected signs and magnitudes, respectively, to determine if the hypotheses may be falsified or the retrospective predictions rejected as insufficiently accurate.

Unfortunately, the failure to use dimensions consistently and correctly makes it almost impossible to prevent untenable and unreasonable assumptions from entering into the mathematical and econometric models undetected. Such assumptions, of course, render the models so afflicted virtually worthless. They make possible, as we have seen, such indefensible results as differentiated goods that are identical. (Or, to amend slightly a remark of Coase (1988, p. 185), "In my youth it was said that what was too silly to be said may be sung. In modern economics it may be put into [dimensionless] mathematics".) Such clearly untenable results go unchallenged because the dimensionless mathematics obfuscate, rather than illuminate, the analysis and also because some are intimidated by the mathematics.

Certainly, the problems exposed in the examples could have been avoided had dimensions been used consistently and correctly. Whether, then, the author could have developed a tractable model is a different matter. Nevertheless, it is clear, at least to the present author, that anytime the choice is between a dimensionless, tractable, mathematical model or none at all, the latter is by far the better choice.

None of this should be taken to say that the author whose work provided the example did not have valuable insights into the economic activities with which he was concerned–he well may have. However, that must be determined independently of his mathematical model, as it provides no valid support for his argument.

Conclusions

The economics profession has attempted to achieve the degree of success in understanding, explaining, and predicting events in the social world that physicists and engineers have achieved in the natural world by emulating their methods; i.e., using mathematical and statistical analyses to model, understand, and explain, the relevant phenomena. However, in so doing, economists have failed to emulate physicists and engineers in one essential aspect of their work: the consistent and correct use of dimensions. This is an abuse of mathematical/scientific methods. Such abuse invalidates the results of mathematical and statistical methods applied to the development and application of economic theory.

Neither is this problem a thing of the past, nor is it one confined to lesser or fringe venues. Rather, it is a continuing problem and one found in the

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leading mainstream journals (and textbooks). Because young minds are formed by such materials, future generations of economists are being brought along in a faulty tradition. And, unless and until this changes, and economists consistently and correctly use dimensions in economics, if such is possible, mathematical economics, and its empirical alter ego, econometrics, will continue to be academic games and "rigorous" pseudosciences. However, if for no other reason than the influence of the economics profession on governmental policies, such games and pseudosciences are not without their costs in the real world.

This is not to say that there have not been advances in economic understanding by the neoclassicals, but rather to argue that mathematics is neither a necessary nor a sufficient means to such advances. Whether it even is, or can be, a valid means to such advances is a different issue. What is certain, however, is that mathematics cannot possibly be a valid means unless and until it is used properly. Among other things, that means that dimensions must be used consistently and correctly.

ADDENDUM

There is an ongoing debate in the literature as to whether Austrian School economists should attempt to publish in mainstream journals or rather in nonmainstream journals created specifically for the purpose of providing a venue for explicitly Austrian work. Among the most recent additions to this literature are Rosen (1997); Yeager (1997, 2000); Vedder and Gallaway (2000); Laband and Tollison (2000); Backhouse (2000); Block (2000); and Anderson (2000).

One issue centers about type one errors; i.e., the exclusion of explicitly Austrian work, regardless of quality, from mainstream journals, and, *a fortiori*, top-tier, mainstream journals. As such, it is implicitly assumed that Austrians should aspire to publish in such journals.

Perhaps of more importance is the issue of whether Austrians should aim to publish in such journals. This raises the spectre of type two errors; i.e., the inclusion in such journals of material that should have been excluded for lack of quality.

Yeager (1997, pp. 159-64; 2000) attacks the concept of the so-called "marketplace for ideas." The marketplace for ideas in economics is taken to constitute the top-tier mainstream journals. Whereas, the exclusion of Austrian work from these journals is taken to mean that Austrian ideas have failed the test of the market, Yeager points out the perversities of such a test. He argues that Austrian economics is not valueless merely because it is uncompetitive in that market and thus, implicitly, Yeager would agree that there should be a venue for good Austrian work that cannot be published therein. However, he does not claim that such journals have no value, and can be interpreted as saying that Austrians should publish in top-tier, mainstream journals when

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possible and in specifically Austrian venues only as a fallback position. Vedder and Gallaway (2000) also can be reasonably read to arrive at the same conclusion.

Block (2000, p. 55), presumably on the grounds of type two, as well as type one, errors challenges the very legitimacy of the editors of mainstream journals: "One difficulty is that Vedder and Gallaway *unnecessarily concede* to the very editors they accuse of bias against Austrians *a modicum of legitimacy*" (emphasis added). Anderson (2000) makes explicit the argument that these journals commit many and serious type two errors. Whether he would also prefer the strategy of publishing in specifically Austrian journals as a fallback is not clear. However, Block (2000, pp. 55-56) states that "[i]n [his] view, the leading economic journals are the Austrian ones."

Most of the debate in the literature cited above concerns anti-Austrian bias of the editors and referees of mainstream journals. *In what follows I question the competence of the editors and referees of top-tier, mainstream journals on other grounds and, therefore, the desirability of attempting to publish in them.* Specifically, I challenge the competence in mathematics of these editors and referees and make the case by relating a real life case. The facts, as revealed in the referees' reports (on a paper submitted for consideration for publication), the response thereto, and the co-editor's follow-up correspondence, prove beyond any doubt that the referees and co-editor were incompetent to judge the paper. And this is not a case of opinion, theirs versus mine; no, it is a clear and indubitable case of their commission of mathematical errors.

One requirement for the proper use of mathematics is the correct use of dimensions/units.[18] For example, dimensional analysis is used in physics and engineering to insure the consistency of the relationships in an equation. The economic variables one sees in the mathematical and statistical models ubiquitous in economics (Backhouse 2000) always involve dimensions. However, dimensions/units are rarely used in economics, and dimensional analysis virtually never. Consequently, I submitted a paper,[19] on this subject to a leading English language economics journal. The paper was an indirect attack on the use of mathematics in economic theory. It maintained that if one uses mathematics in economics one must do so correctly.[20] It demonstrated that a dimensional analysis of production functions, specifically, the Cobb-Douglas,

because, "Z is an aggregate technology index, whose growth rate is assumed to follow an independently and identically distributed (i.i.d.) process $\{\eta_t\}$, with $\eta_t \sim N(o, s_z^2)$. Formally, $Z_t = Z_{t-1} \exp(\eta_t)$."

[16]We are given that: $\varphi = \alpha\theta + \alpha(1-\theta)(1+\sigma_n)/(1+\sigma_u)$. We know that $\theta \in (0, 1)$, σ_n, σ_u, and α are positive, dimensionless, constants. (See note 3.) Therefore, φ must be a positive, dimensionless constant.

[17]It is true that time (t) squared does have a meaning in the world of the natural sciences, such that the dimensions of acceleration are distance/t^2. But that does not in any way help us find meaning for t^2 in the world of the social sciences.

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yields meaningless or economically unreasonable, and inconstant, dimensions. It then provided two examples of the consequences of the failure to use dimensions and dimensional analysis in economics, one microeconomic and one macroeconomic, criticizing articles in then-very-recent issues of that same journal. Application of dimensional analysis resulted in the conclusion that both models were untenable and nonsensical.[21]

The paper was rejected. Included with the letter of rejection were the reports of three referees. The following are excerpts from these reports along with brief proofs of the errors therein.

> From referee #1's report:
>
> A "defect" in economic analysis is proposed, in that equations do not properly account for units, and that two sides of equations used generally in economics are therefore inconsistent. It is claimed that this defect is not present in the physical sciences, such as physics, and that this defect invalidates most formal economic modeling. The "defect" is best illustrated by an example taken from the paper, which I shall detail next. I shall then show that this "defect" is also present in physics by using illustrations from a random book off of my shelf that has some examples of simple physical systems. Then I shall argue that this is, in fact, not a defect at all.
>
> From referee #2's report:
>
> Dimensional analysis can only be applied to *laws*.
>
> A case in which this [dimensional] analysis made sense in economics was its application to Fisher's relation of exchange: $MV=PT$. This is one of the few examples in economics that comes closest to a law. One result of dimensional analysis is that there is something odd with this equation. The left part does contain a time dimension, while the right side doesn't. This is not something new and can be found in any textbook.
>
> And, from referee #3's report:
>
> There is no question that the lack of dimensional consistency is pervasive throughout mathematical economics. However, this paper does not make clear why this lack of dimensional consistency is problematical. The lack of dimensional consistency is not so much a problem in and of itself. . . .

Compare the referees' statements with the following taken from two leading (basic) physics textbooks.

[18]This is not a difficult thing, and, in fact, there is published work on the subject of dimensional analysis. For an example, see the appendix in Reddick and Miller (1955).

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> Dimensional analysis is used to check mathematical relations for the consistency of their dimensions . . . *[i]f the dimensions are not the same, the relation is incorrect.* (Cutnell and Johnson 2001, p. 6; emphasis added)

> An equation must *always* be dimensionally consistent; this means that two terms may be added or equated only if they have the same units. . . . When a problem requires calculations using numbers with units, the numbers should *always* be written with the correct units, and the units should be carried through the calculation as in the example above. This provides a useful check for calculations. *If at some stage in the calculation you find that an equation or expression has inconsistent units, you know you have made an error somewhere.* (Sears, Zemansky, and Young 1987, p. 7; emphasis added)

Is it possible to believe that anyone with even the most elementary training in mathematics could make the statements made by these referees? This is incredible! Are the referees innumerate? How else to explain the foregoing? But there is more.

> Also from referee #1's report:
>
> The details are not very important, but the solution to the problem [of simple harmonic motion] posed [Spiegel 1967, p. 186] is $x = 1/3 \cos 8t$, where x is distance measured in feet (the deviation from the equilibrium position of the weight) and t is time measured in seconds. So exactly what kind of conversion constant [sic] do you [Barnett] want to use to convert time into [sic] distance? It is evidently not a constant, since it must be passed through the cosine expression [sic] (similar to passing units of labor or capital through the exponents [in $Q = AK^{\alpha}L^{\beta}$] above.)

But of course the details are important, because for this referee the devil is in the details. A formula, $x = A \cdot \cos \omega t$, for the displacement in simple harmonic motion can be found in Cutnell and Johnson (2001, p. 278). In this formula: x is the displacement, measured in units of length; A is the amplitude of the simple harmonic motion, also measured in units of length; ω is the constant angular speed, measured in radians/second (rad/sec); and, t is the elapsed time, measured in seconds (sec). Consequently, ωt has the dimensions rad.

Restating the formula, $x = A \cdot \cos \omega t$, with the appropriate units attached, and using feet (ft) as the unit of length, yields: $x[\text{ft}] = A[\text{ft}] \cdot \cos \omega[\text{rad/sec}] \cdot t[\text{sec}]$. Canceling the sec on the right-hand side yields: $x[\text{ft}] = A[\text{ft}] \cdot \cos \omega[\text{rad}]t$.

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However, a radian is a dimensionless measure of a plane angle[22] (1 rad = $180°/\pi \approx 57.30°$). Therefore, the only unit that "must be passed through the cosine expression" is an (plane) angular measure. Of course, converting plane angular measures, whether radians or degrees, to a pure, i.e., dimensionless, number is precisely what the trigonometric operators, cosine included, do. Consequently, the equation $x = A \cdot \cos \omega t$ has the unit feet on both sides.

Compare the referee's specific equation, $x = 1/3 \cos 8t$, with the generic form, $x = A \cdot \cos \omega t$. The correspondences between the terms in these equations are: $x = x$; $A = 1/3$; magnitude of $\omega = 8$; and, $t = t$. Use these correspondences to restate the equation, $x = 1/3 \cos 8t$, with the appropriate units made explicit, as: x[ft] = 1/3[ft] cos (8[rad/sec] $\cdot$ t[sec]). Cancel the sec on the right-hand side to obtain: x[ft] = 1/3[ft] cos 8[rad] $\cdot$ t, where the term cos 8[rad] $\cdot$ t is dimensionless. Then, as must be the case, the units on the right-hand side are identical to the units on the left-hand side; to wit, in this case, feet. Obviously, the referee's statement is erroneous.

This example brings to mind the term *idiot savant*. No doubt, this referee knows a great deal of pure mathematics; but, does he know anything at all about applied mathematics or physics? On the evidence he provides in the foregoing excerpt, the answer, at least with respect to harmonic motion, is a resounding, "NO!"

> And, yet again, from the same referee #1:
>
> > If one wants an example from physics not involving time, try p. 97 [Spiegel 1967], where there is an example concerning thermal conductivity in pipes. The solution is $U = 699 - 216 \ln(r)$, where r is distance in centimeters and U is temperature in degrees. Now what kind of conversion factor do you want to use to convert distance into degrees? I conclude that physics contains the same "defect" when certain systems are examined.

Once again, this referee exhibits his ignorance of applied mathematics and physics, at least with respect to thermodynamics. In fact the dimensions on *both* sides of the equation $U = 699 - 216 \ln(r)$ are degrees centigrade. As the proof is somewhat lengthy it is included as an appendix.

The following, with emphasis added, is the corpus of the co-editor's letter of rejection that accompanied the referees' reports excerpted, above.

> I enclose three *thoughtful* reports on your manuscript. The referees, while sympathetic, unambiguously recommend rejection. *I agree with these assessments* and must reject your manuscript.
>
> The referees on occasion adopt a somewhat harsh tone. I hope you can see that they took the refereeing responsibility very seriously and have written *thoughtful* reports. They labored to understand your thinking, and the

[19]That paper, with minor, nonsubstantive editorial changes, constitutes the body of the present article, save that, at the suggestion of a referee, one (1) example was removed,

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> occasional harsh word is the consequence of frustration, one that I felt in reviewing your manuscript as well.
>
> The [journal] receives about 1000 manuscripts per year, and publishes less than ten percent of these. As a consequence, I am forced to reject many quite good manuscripts. Thank you for submitting your paper to the [journal]. I am sorry my response could not be more satisfying.

I submitted a *12-page* reply to the referees' reports in which numerous errors were called to the co-editor's attention, specifying, for each error, the nature thereof, and providing, for each, a detailed proof of the error.

The co-editor responded to my reply with a letter dated February 1, 2001, the corpus of which follows.

> I am responding to your letter of Jan. 12, 2001.
>
> Evidently you could not see past the tone of the reports to the substance of the reports. Unfortunately, reading *your diatribe on the referees' errors has not convinced me of the error of their ways.* The case of the Cobb-Douglas production function is quite clear. Just because you think that the units associated with C-D are unnatural doesn't make it so. *Moreover, the referees are right about the units required to rationalize physics*–distance squared or log(temperature) makes no more sense than the square root of manhours. The units are what they are, and certainly you can't really think the Cobb-Douglas production function is logically inconsistent. Like a law of nature, a production function is whatever it is.
>
> As you surmised, I am not going to reopen the file. *At the very minimum, you have failed to convince three referees and one editor of the merit of your approach.* A great deal more effort into communicating the results is going to be necessary, I suspect, to sell this work to any journal. You could try *Economics & Philosophy*.

The editor clearly states that he "agree[d] with these [referees'] assessments," that the reports were "thoughtful," that my "diatribe" did not "convince [the editor] of the error of [the referees'] ways," and that, "[a]t the very minimum, you have failed to convince three referees and one editor of the merit of your approach." My reply incorporated the material included above and in the appendix; moreover, it went into greater detail. How, then, could the editor reach the conclusions he did? Three referees and an editor at a top-tier journal and all innumerate? Never in my wildest dreams did I think that an editor and referees for one of the most prestigious English-language economics journals could be so ignorant in a matter of basic mathematics, much less that they would commit such to paper, where it cannot be denied and can, and is being preserved for posterity.

This brings me back to the basic issue, the desirability of specifically Austrian journals. Given the extent of the mathematization of economics, it is critically important that mathematics, if used at all, be used correctly in

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economics. Therefore, the subject matter of my original paper is very important. Moreover, if my paper is correct—if, in fact, mathematics is abused/misused in economics if for no other reason than that articles in at least one top-tier journal cannot pass the test of dimensional analysis—the paper is worthy of publication in an important venue. The fact that it was rejected by referees and an editor incompetent to the task because of a demonstrated lack of understanding of the basic mathematics of dimensions, the very subject matter of the paper, provides a sufficient reason to have alternative outlets, i.e., specifically Austrian journals, available.[23] Moreover, I think it highly improbable that referees of lesser mainstream journals would succeed where those at a more prestigious journal failed. Therefore, if my paper, and others similar in that they are counter to the prevailing orthodoxy, are to be published, it must be in journals receptive to heterodoxy. Moreover, I would not even consider submitting the instant paper to a mainstream journal of any rank. I cannot imagine such an attack against a top-tier mainline journal ever being published in such a journal. And yet, if in fact referees and editors at top-tier mainstream journals are incompetent in any relevant area this is important for the profession to know.

I conclude, therefore, that there is a need for specifically Austrian journals, not only because of the bias of mainstream journals, but also because Austrians should not have to subject their work to referees and editors incompetent in the very area of their supposed expertise.

APPENDIX

Referee #1 took the equation, $U = 699 - 216 \ln(r)$, from an example in Spiegel (1967, pp. 97–98.) (Please note that I used a different edition, Spiegel (1981, pp. 103-04).) The example is formulated as a problem with three parts. The "[s]olution . . . $U = 699 - 216 \ln(r)$," which the referee took from the book, is but the solution to one part thereof. I have reproduced the relevant portion of the example immediately below. (Emphasis in original.) Subsequently, I restate, in expanded form, the example in a way that explicates the referee's error.

> The amount of heat per unit time flowing across an area A is given by
>
> $$q = -KAdU/dn \qquad (3)$$
>
> The constant of proportionality K, used above, depends on the material used and is called the *thermal conductivity*. The quantity of heat is expressed in *calories* in the cgs system, and in *British thermal units*, *Btu*

solely because it involved the Leontief, fixed-coefficient, production function that the referee thought to be "of little general interest and of no interest to readers of the *QJAE*."

[20]Although Austrians should only use mathematics when doing history (Block 2000, p. 48), when they do use it this dictum applies to them as well.

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in the fps system. [Because of the confusion that arose from the use of "pound" as a unit of mass and as a unit of force, in the modern version of the fps (foot-pound-second) system, the BE (British Engineering) system, the slug, not the pound, is the unit of mass.] Consider now an illustration using the above principles.

A long steel pipe, of thermal conductivity $K = 0.15$ cgs units, has an inner radius of 10 cm and an outer radius of 20 cm. The inner surface is kept at 200° C and the outer surface is kept at 50° C. (a) Find the temperature as a function of distance r from the common axis of the concentric cylinders. (b) Find the temperature when $r = 15$ cm. (c) How much heat is lost per minute in a portion of the pipe which is 20 m long?

MATHEMATICAL FORMULATION (Spiegel 1981, pp. 103-04). It is clear that the isothermal surfaces are cylinders concentric with the given ones. The area of such a surface having radius r and length l is $2\pi rl$. The distance dn is dr in this case. Thus, equation (3) can be written

$$q = -K(2\pi rl)dU/dr \tag{4}$$

Since $K = 0.15$, $l = 20$ m $= 2000$ cm, we have

$$q = -600\pi r\, dU/dr \tag{5}$$

In this equation, q is of course a constant. The conditions are

$$U = 200° \text{ C at } r = 10,\ U = 50° \text{ C at } r = 20 \tag{6}$$

SOLUTION. Separating the variables in (5) and integrating yields

$$-600\pi U = q \ln r + c \tag{7}$$

Using the conditions (6), we have $-600\pi(200) = q \ln 10 + c$, $-600\pi(50) = q \ln 20 + c$ from which we obtain $q = 408{,}000$, $c = -1{,}317{,}000$. Hence, from (7) we find

$$U = 699 - 216 \ln r. \tag{8}$$

EXPANDED RESTATEMENT. The foregoing material is restated with the units, in brackets, explicitly attached to the algebraic symbols for the variables. The dimensions of thermal conductivity are units of: energy/(time • distance • thermodynamic temperature). Therefore, K, the thermal conductivity of the pipe, is, in (cgs) units, cal/(sec • cm • °C). The units of the other relevant variables are: cm for l, dr, and r; °C for dU and U; and, cal/sec for q.

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Rewriting equation (4) yields:

$$q[\text{cal/sec}] = -K[\text{cal/(sec} \cdot \text{cm} \cdot {}^\circ\text{C})] \cdot (2\pi r[\text{cm}]l[\text{cm}]) \cdot dU[{}^\circ\text{C}]/dr[\text{cm}] \qquad (4')$$

Substituting the values for K and l yields:

$$q[\text{cal/sec}] = -0.15[\text{cal/(sec} \cdot \text{cm} \cdot {}^\circ\text{C})] \cdot 2\pi r[\text{cm}]2000[\text{cm}] \cdot dU[{}^\circ\text{C}]/dr[\text{cm}] \qquad (5')$$

The cm unit and the °C unit in the denominator of the dimension of K cancel the cm unit in the numerator of the dimension of l and the °C unit in the numerator of the dimension of dU, respectively. Note particularly that because the units of r and dr are identical, cm, and because r is in the numerator and dr in the denominator, the units of the variables r and dr cancel out, and all that is left of these variables are their magnitudes; the algebraic symbols of these variables no longer have units attached. Therefore, canceling units yields:

$$q[\text{cal/sec}] = -600[\text{cal/(sec)}] \cdot \pi r \cdot dU/dr \qquad (5'')$$

At this point, the only units that have not canceled out are cal/sec on both sides of the equation.

Rewriting (5″) to put it in integrable form yields:

$$-600[\text{cal/(sec)}]\pi dU = q(\text{cal/sec})\ dr/r. \qquad (5''')$$

The integration of dr/r yields ln r, but the algebraic symbol for the variable r in the term ln r has been shorn of its units and only its magnitude remains, and, therefore there are no units to be operated on by the ln operator. Similarly, the integration of dU yields U, but the algebraic symbol for the variable U has been shorn of its units and only its magnitude remains. The solution to (5‴), then, is:

$$-600[\text{cal/(sec)}]\pi U = q[\text{cal/sec}] \ln r + c \qquad (7')$$

Recall the conditions (6) i.e., $U = 200°$ C at $r = 10$ [cm] and $U = 50°$ C at $r = 20$ [cm], while remembering that r refers only to the relevant magnitudes at this point; the cm appear in brackets only as a reminder of the dimensions that r had prior to their being canceled in the equation, $q[\text{cal/sec}] = -K[\text{cal/(sec} \cdot \text{cm} \cdot {}^\circ\text{C})] \cdot (2\pi r[\text{cm}]l[\text{cm}]) \cdot dU[{}^\circ\text{C}]/dr[\text{cm}]$. Then, substituting these conditions into (7′), and solving for q and c yields: $q = 408{,}000$ cal/sec and c = -1,317,000 cal/sec. Note: the units of c are, necessarily, cal/sec, else dimensional analysis would yield inconsistent units, an absolutely certain sign of error, to wit: an incorrect relation.

In order to solve for U including, the appropriate units, rewrite (7′) (substituting $q = 408{,}000$ cal/sec for q and c = -1,317,000 cal/sec for c) as:

$$-600[\text{cal/(sec} \cdot {}^{\circ}\text{C})]\pi\ U[{}^{\circ}\text{C}] = 408{,}000\ [\text{cal/sec}] \ln r - 1{,}317{,}000\ [\text{cal/sec}] \qquad (8')$$

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Isolating the U term yields:

$$U[°C] = (408{,}000\ [cal/sec] \ln r - 1{,}317{,}000\ [cal/sec])/(-600[cal/(sec \cdot ºC)] \cdot \pi) \quad (8'')$$

As the units cal/sec appear in every term in the numerator and in the denominator of the right-hand side of 8″, they may be canceled, yielding:

$$U[°C] = (408{,}000\ [°C] \ln r - 1{,}317{,}000\ [°C])/(-600\pi) \quad (8''')$$

or

$$U[°C] = 699[°C] - 216[°C] \ln r. \quad (8'''')$$

Because, as previously shown, $\ln r$ is dimensionless, it is obvious that the dimensions on both sides of equation (8″″) are degrees Celsius, and, therefore, identical, as must be the case.

However, equation (8″″) is the very solution (*cum* appropriate units) that the referee cited in his report, to support his position.

Therefore, both Referee 1's undeniable implication that the units on the different sides of the equation, $U = 699 - 216 \ln r$, are not the same, in that the units on the left-hand side are degrees Celsius and the units on the right-hand side are centimeters, and his conclusion "that physics contains the same 'defect' [i.e., the failure to properly account for units and, therefore, the inconsistency between the two sides of equations] when certain systems are examined, are incorrect. *QED.*

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A Critique of Mises's Theory of Monopoly Prices

Diana Costea

In his *magnum opus*, *Human Action*, Ludwig von Mises deduces, in a value-free manner, economic laws from the *a priori* axiom of human action. His achievement is also a defense of the market economy which is considered, from a utilitarian perspective, as the only framework in which it is possible to realize "the objective of social cooperation . . . of the greatest happiness of the greatest number" (Mises 1998a, p. 830). From whence springs the increase in the range of means suitable to the fulfilment of individuals' ends? It comes from voluntary human interactions (exchanges) under the auspices of the division of labor and specialization. This allows for the emergence of a widely used medium of exchange, i.e., money, and thus also economic calculation.

In the larger picture of the market as a harmonizer of the market participants' interests, Mises identifies a "failure": monopoly prices. He describes this as a situation in which the pattern of production chosen by the monopolist does not allow for the complete satisfaction of the most important wants of consumers. The unique owner of a monopolized resource withdraws a part of the supply of goods from the market, or restricts his production from the very beginning in order to obtain higher net proceeds than otherwise.

Mises identifies three conditions required for the emergence of a monopoly price. The first refers to the "monopoly of supply," as the single potential supplier of the good on the market. The second condition is the inelasticity of market demand:

> The reaction of the buying public to the rise in prices beyond the potential competitive price, the fall in demand, is not such as to render the proceeds

Diana Costea is a Ph.D. student and teaching assistant of economics at the University of Bucharest. I would like to thank Guido Hülsmann, Walter Block, Jeffrey Herbener, Cristian Comanescu, Joseph Salerno, and Nikolay Gertchev for helpful comments and discussions. Comments are welcome at diana_costea@yahoo.co.uk.

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> resulting from total sales at any price exceeding the competitive price smaller than total proceeds resulting from total sales at the competitive price. (Mises 1998a, p. 355)

To put this another way:

> If conditions are such that the monopolist can secure higher net proceeds by selling a smaller quantity of his product at a higher price than by selling a greater quantity of his supply at a lower price, there emerges a *monopoly price* higher than the potential market price would have been in the absence of monopoly. (Mises 1998a, p. 278)

Third is "a special condition," "the monopolist's ability to discover such prices" (Mises 1998a, p. 359). This is the condition that differentiates the Misesian approach from the neoclassical analysis, the latter being based on the implicit assumption regarding knowledge of the demand curve (Coase 1937).[1]

Rothbard's critique of the relevance of the concept of monopoly prices on the free market, including here the Misesian approach, is very compelling.[2] He demonstrates that in the context of the private-property order, the operational criterion for distinguishing between market price and monopoly price is entirely lacking (Hoppe 1990, pp. 249-63). All the particular characteristics of the so-called "monopoly price" are ordinary features of market prices: the comparative restriction of production in order to achieve a higher price and the expectation of an elastic demand above the selling price. Rothbard emphasizes that the meaningful concept on the market is that of "self-sovereignty over his person and property" (Rothbard 1993, p. 562).[3] This is closer to the meaning of the market as a private-property order. He relegates the existence of "monopoly prices" to the hampered market, where the difference between a market price and one resulting from privilege received from the state is logically coherent.

The goal of our inquiry here is to add weight to the Rothbardian critique of Mises's theory of monopoly prices. We do so by highlighting the inconsistencies of the latter's treatment of monopoly prices and by arguing that it is incompatible with his own general framework of praxeological analysis. In the first part, "Welfare Arguments Based on Value Theory," we discuss the implicit interpersonal comparisons of utility Mises uses in order to claim that consumers are hurt by monopolistic restriction of production. We argue in the second part, "Propertarian Monopoly Theory," that monetary revenues cannot

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[1]Coase acknowledged the problem, "that the producer does not know whether his estimate of the demand curve is correct or not."

[2]See Rothbard (1993, chap. 10) and, for further development along Rothbardian insights, also Block (1977). See also Hoppe (1989, chap. 9).

[3]For the concept of "consumer sovereignty," see Persky (1993). For a defense of Misesian theory of monopoly prices, see Kirzner (2000, pp. 165-79).

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convey information about comparative consumers' welfare and that the correct approach to deal with welfare statements is to point out the framework of property rights under which actions are undertaken. Section three, "Monopoly Prices and Information," is devoted to the proper context of a possible identification of monopoly prices. We focus on this goal by questioning the compatibility between two of the Misesian conditions for the emergence of monopoly price. The final section, "Restriction of Production and Reservation Demand," examines in what sense the restriction of production allegedly used by the monopolist to extract more money than otherwise from consumers could be the element which allows us to differentiate between competitive prices and monopoly prices. We will argue there that Mises changes the framework of discussion from the real market to an equilibrium construct in order to arrive at a meaningful distinction between market prices and "monopoly" prices.

Welfare Arguments Based on Value Theory

Embarked upon the task of showing that consumers are hurt by the "monopolistic" restriction of production policy, Mises makes an implicit value judgment and also assumes the possibility of an interpersonal comparison of utility. If the producers restrict the quantity produced and offered on the market and a smaller quantity of this particular good is produced, then a comparatively higher quantity of relatively nonspecific factors of production will be available for other products. This means that different consumers will enjoy a higher quantity of other goods offered on the market.

By deploring the perceived reduction in quantity produced, Mises overlooks the fact that a higher quantity would have diminished the quantity of other goods. For instance, using an additional quantity of lumber to produce chairs will necessarily diminish the number of tables which could have been otherwise obtained. This allocation of resources will bring about a lower price of chairs and a higher price of tables than otherwise. This scenario is a common one in our world of nonspecific scarce resources. In the second quote by Mises above, he implicitly considers that the marginal utility of certain individuals derived from the consumption of the would-be additional production is higher than the present marginal utility of other individuals who are benefited from the increased supply they enjoy. The relative wealth position of the two categories of producers would also have been different (Mises 1981, p. 348). As the analysis runs in terms of a market economy without an aggressive interference by the government, we cannot defend the case of the first category against the second without making an implicit value judgment.[4]

[4]See Armentano (1990, pp. 28-29) for a discussion about the impossibility of "discovering, comparing, measuring" the resulting gains and losses. According to Egger (1995, pp. 191-92):

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Mises recognizes this fact, but explains his position by saying that the goods produced with the freed factors of production are less important, from the consumers' point of view, than the goods whose production was purposefully restricted:

> Capital and labour, set free by the restriction of production, must find employment in other production. . . . Thus against the smaller production of the monopolized goods one must set the increased production of other goods. But these, of course, are less important goods, which would not have been produced and consumed if the more pressing demands for a larger quantity of the monopolized commodity could have been satisfied. *The difference between the value of these goods and the higher value of the quantity of the monopolized commodity not produced represents the loss of welfare which the monopoly has inflicted on the national economy.* Here private profit and social productivity are at variance. (Mises 1981 p. 348; emphasis mine)

What criterion could we use in order to arrive at the conclusion that the units of the monopolized good which would have been produced instead are more valuable than the goods actually produced with the liberated factors of production?

Mises frequently speaks about the fact that entrepreneurs, by looking for the highest net proceeds, respond to the most important desires of the consumers (Mises 1998a, p. 337). Without questioning for the moment the legitimacy of this criterion, we wonder why, in the case of monopoly prices, this criterion no longer applies? Mises defined monopoly prices (the second condition for the emergence of monopoly prices) as prices at which net proceeds are higher than otherwise. Let us accept the view that higher monetary incomes are the expression of the better satisfaction of consumers' desires and apply it to monopoly prices. The result is that the monopolistic "restriction of production" assures a rational combination of resources, a higher satisfaction of consumer preferences than otherwise, because consumers are ready to give a higher amount of money for the restricted production.

Thus, if, in the paragraph quoted above (Mises 1981, p. 348), "value" means total (monetary) proceeds obtained by sellers, it is contradicted by Mises's second condition of the emergence of monopoly prices, i.e., inelasticity of the consumers' demand. According to this condition, the monopolist allocates resources in order to obtain the highest net proceeds from the present use of the factors of production. This means that, by restricting production,

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> the consumers are worse off, but the monopolist is better off. How can we conclude that welfare is lost? . . . the simple fact that the consumers preferred the previous state does not establish their right to it; in fact, the monopolist–if his position was achieved and is maintained by voluntary exchange–does have the right to assert the higher price and lower quantity.

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he obtains higher net proceeds than otherwise, i.e., he directs resources to their most value-productive use. The complementary factors of production, which could have been devoted to an increase in the production of the "monopolized good," are allocated to other, more remunerative activities. If our "monopolist" could obtain higher net proceeds by using these complementary factors of production to produce yet a higher quantity of the monopolized good, he would have done it. Let us assume, for instance, that the producer must decide between the following patterns of production: either producing 80 units of the monopolized good with anticipated net proceeds of 1,000, or producing 100 units with expected proceeds of 900. From an *ex ante* point of view, the entrepreneur estimates not only the prices of the good, but also the prices of complementary factors of production. The above situation could be interpreted as follows: there are other, more remunerative productive uses for the additional quantity of complementary factors of production necessary for a potential increase in production. The monopolist cannot afford to buy them, given his resources and his anticipations, unless he is willing to make a gift to his potential consumers. This means that other entrepreneurs bid up the prices for these factors of production because they anticipate a higher value-productive use in their own enterprises. This happens at every moment on the market: factors of production are obtained by capitalist-entrepreneurs who anticipate a higher value-productivity of factors of production in the production of goods and give a higher price for them. This is not at all associated with a "market failure." To the contrary, this method assures the satisfaction of the most important desires of consumers.

On the other hand, let us suppose that "value" means satisfaction derived from consumption of the two different kinds of goods by different individuals on the market. If so, Mises's argument involves a clear case of an aggregation of subjective valuations and interpersonal comparison of subjective values. But why should only a fraction of the present consumers be taken into account and be considered worse off as a result of "monopolistic" activity? Why not also take into account future potential consumers if "consumers" are our only concern? By restricting present production, the monopolist is able to provide more goods in the future than otherwise. Thus, other, or the same, consumers would be better off at that latter time. This is tantamount to saying that the part of the monopolized input unused by its owner has an opportunity cost (the anticipated future revenues), which must also be included in his economic calculation.

So far we have assumed that the criterion of monetary net proceeds allows us to differentiate among the lines of production which better satisfy consumers. Now let us examine if this criterion is sufficient to establish the desirability of the goods that are given up, from the consumers' point of view. We will first examine this issue from the individual's perspective, and then at the level of the entire market.

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We must first renounce the fallacy that price measures the value of goods exchanged. Mises rejected any notion of measuring value by the money prices people are willing to give up in order to obtain desired goods:

> People buy and sell only because they appraise the things given up less than those received. Thus the notion of measurement of value is vain. An act of exchange is neither preceded nor accompanied by any process which could be called a measuring of value. (Mises 1998a, p. 205)[5]

As the Austrians emphasize, money prices are exchange ratios. There is no previous act of individuals that attaches value to goods received and given in exchange (Hülsmann 2002; White 1995). From an act of exchange all that the external observer can deduce is that the individual prefers the good received to that given up. Nor is the situation on the monetary side any different. Money is nothing else than an economic good which serves as medium of exchange. Individuals could prefer a good to a certain amount of money, i.e., they express greater preferences for the good than for money, or the goods that this quantity of money could purchase.

The price on the market is determined by the preferences of the consumers and the stock (or supply, if we take into account the possibility of a reservation demand on the part of the producer). At the market price, each individual agrees to perform the exchange as long as the sum of money (the price) ranks lower on his value scale than the good he buys.

"Present" prices are formed at different points in time in the immediate past. Of course, the scales of value of individuals can change from one moment to another. This fact precludes us from deriving from the higher amount of money spent for a given good the conclusion that this good is more highly valued than another, cheaper alternative. Assume, for instance, that according to my value scale I am willing to give \$1,200 for good x and \$1,000 for good y, but the market prices are \$800 for the first and \$900 for the second. I am therefore a supramarginal buyer for both goods. By purchasing them, I demonstrate only that at different points in time I prefer the good x to \$800 (or to the goods which could be bought with this sum) and the good y to \$900. But an external observer cannot say that the first good is ranked lower or higher on my value scale. He cannot even conclude that I have

[5]On the issue of consumer's equilibrium there is a qualitative distinction between the neoclassical formula $MU_1/P_1 = MU_2/P_2$ and the Austrian approach. The Austrians' critique refers to the irrelevance of the concept of indifference for the explanation of human action. They point out that every choice involves a fundamental inequality between the ranks of the two goods exchanged. The neoclassical formula neglects also the purely ordinal character of preferences, in which case the idea of dividing ranks by cardinal numbers is simply meaningless. I am indebted to Guido Hülsmann for bringing to my attention this issue. For an extensive critique of the neoclassical view see Hülsmann (1999), and Block (1999).

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demonstrated preferences ranked *on the same* value scale.[6] Therefore, neither the unit price, nor the total expenditure on a good by an individual measures its "value" to him; it follows that we cannot use amounts of money as a welfare criterion, to perform comparisons between the effects of different actions on different people's welfare.

At the level of the market for a product, the welfare information which could possibly be conveyed by higher net proceeds is even less accurate than at the individual level. In the latter case, at least we have a choice of the individual, and thus a scale of value demonstrated in action. This is definitely not true for the market demand; we cannot construct a scale of value for all consumers taken together, and even if we could arrive at a certain ranking of the goods desired by these individuals, this would be the result of individual exchanges made at different moments, not the demonstrated preference of a supra-individual acting entity. There would also be another problem: what criterion do we have to use in order to rank the goods bought by people in a community? The quantity exchanged, total net proceeds, or the number of people who have bought the particular item, etc.? Moreover, the attempt to arrive at a unique ranking of goods at the level of the community (irrespective of the criterion chosen) is useless, as the attainment of this goal, *per impossible*, will necessarily depend on subsequent voluntary interpersonal exchanges.

The fact that we cannot rely on monetary proceeds to compare consumers' satisfactions associated with the consumption of goods is even more evident in the case of inelastic demand. In this case, the restriction of production leads to sales at a higher unit price, and the total net proceeds are therefore higher than from selling a larger quantity at a lower price.[7]

In this section we have shown that Mises embraces the nonscientific method of interpersonal comparison of utility in order to support his claim that "monopoly prices" would have a negative impact on consumers' welfare. Mises does not succeed in proving, by using value theory, that consumers are hurt by the "monopolistic" restriction of production.

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[6]We could also mention here the impossibility of measuring pure psychic satisfactions. But even if one day psychology succeeds in finding a unit to measure these satisfactions, the economic analysis remains unchanged. For the same quantity consumed from a good does not always produce the same "amount" of satisfaction even at the level of the individual (Hülsmann 2002).

[7]Fetter (1915, p. 75):

> Sometimes the rise of price through decreased production is so great that the total price of the whole supply is greater than the total price of the larger supply (and vice versa, in case of increased production). This is the paradox of value applying to a whole market and to the buyers' curve of composite demand, rather than to the individual and to his valuation curve.

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Propertarian Monopoly Theory

Mises's theory of monopoly price is based primarily on the reaction of consumers to a price higher than the supposed competitive market price. We previously argued that the loss of welfare that this increase in price causes cannot be revealed by monetary figures.[8] The only other possible approach is the one advocated by Rothbard, in his presentation of the concept of monopoly privileges on the hampered market. Here, the reduction in individual's welfare stems from legal obstacles impeding market participants from following the most highly ranked use of their resources. The issue is discussed in terms of infringement on the property rights of individuals, who are prevented from allocating their property according to its most highly ranked uses, irrespective of the changes in prices:

> [T]he would-be competitors are injured and are forced to accept lower remuneration in less efficient and value-productive fields. The consumers are likewise injured, for they are prevented from purchasing their products from competitors whom they would freely prefer. And this injury takes place apart from any effect of the grant on prices. (Rothbard 1977, p. 37)[9]

Rothbard (1956) acknowledges that we cannot infer propositions concerning welfare by relying on monetary terms derived from value theory. Instead, his welfare criterion employs demonstrated preference based on respect for property rights.

Even as an economist who does not develop a theory of property rights, but assumes the paramount importance of this concept in economic theorizing, Mises could have solved the problem of monopoly prices. Thus, in the chapter dealing with monopoly prices, Mises recognizes that:

> The entrepreneur in his entrepreneurial capacity is always subject to the full supremacy of the consumers. It is different with the owners of vendible goods and factors of production and, of course, with the entrepreneurs in their capacity as owners of such goods and factors. (Mises 1998a, p. 355)

By allowing the capitalist to "withdraw" a certain part of his production from the market, Mises agrees in principle with the meaning of the sovereignty

[8]Kirzner (2000, p. 174) emphasizes the idea that "For Mises, monopoly is identified *at the level of resource ownership*—not, except as a derivative, *at the level of the decisions made by producers*, as in neoclassical theory" (italics his). But the concept of resource ownership has no meaning if it is not accompanied by the decision to produce and supply.

[9]See also Ballvé (1963, p. 54) who states: "But it is impossible to find a single example of a monopoly that has ever existed without official protection" (emphasis his).

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of the individual, in the sense used by Rothbard.[10] Mises implicitly agrees with Rothbard that the owner of the factors of production decides upon the amount of his resources to be used in production. But, in the appraisement of monopoly prices, he makes a sharp distinction between the entrepreneur and the capitalist, in spite of the fact that they could be distinguished only as functions on the market, as Mises says.[11] If the emphasis were put on the entrepreneurial endowment with some property, as a prerequisite of action, i.e., on the concept of capitalist-entrepreneur, the conclusion would have been different. Mises could not have avoided the conclusion that the involvement of entrepreneurs *with their resources* on the market depends on their willingness to participate as much as they want and as long as they want in the social division of labor and cooperation. He would have eventually arrived at the same monopoly theory as Rothbard.

Monopoly Prices and Information

Mises (1998a, p. 359) considers a special condition for the emergence of monopoly prices, "the monopolist's ability to discover such prices," as an additional obstacle to the emergence on the market of this special category of prices:[12]

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[10]Rothbard (1993, p. 560): "the individual is sovereign over his own person and actions and over his own property"; and

> The producer, and producer alone, decides whether or not he will keep his property (including his own person) idle or sell it on the market for money, the results of his production then going to the consumers in exchange for their money. This decision–concerning how much to allocate to the market and how much to withhold–is the decision of the individual producer and of him alone. (p. 562)

[11]Mises (1998a) writes:

> When men in dealing with the problems of their own actions, and when economic history, descriptive economics, and economic statistics in reporting other people's actions, employ the terms entrepreneur, capitalist, landowner, worker, and consumer, they speak of ideal types. When economics employs the same terms it speaks of catallactic categories. The entrepreneurs, capitalists, landowners, workers, and consumers of economic theory are not living men as one meets them in the reality of life and history. They are the embodiment of distinct functions in the market operations. (p. 252)
>
> A capitalist is always also virtually an entrepreneur and speculator. He always runs the chance of losing his funds. (p. 254)

[12]I am indebted to Jeffrey Herbener for this point.

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> The monopolist does not know beforehand in what way the consumers will react to a rise in prices. He must resort to trial and error in his endeavors to find out whether the monopolized good can be sold to his advantage at any price exceeding the competitive price and, if this is so, which of various possible monopoly prices is the optimum monopoly price or one of the optimum monopoly prices. (Mises 1998a, p. 359)

We wonder if the method of trial and error, always time-consuming, could be applied in this case and if it could convey valuable information to the monopolist.

In his approach, Mises implicitly assumes that the relevant factors for the determination of prices (consumers' preferences and the stock of goods offered) remain constant in time. This is an assumption without theoretical foundation, because preferences change over time. It is however a necessary assumption for a meaningful comparison between the two prices that interest us here. For the monopolist (in fact every producer-seller) tries to obtain the highest possible net proceeds given the external conditions he faces: present and future anticipated consumers' preferences, and the competition on the markets for complementary factors of production. The time-consuming movement from competitive to monopoly price has a meaning in the context of the trial and error method only if the restriction of production will meet the same scales of values of consumers. If the preferences of consumers change and they become more willing to consume the "monopolized" good, the price will be higher than otherwise. By merely looking at past prices, the monopolist will not know if they were as high as they were because he succeeded in obtaining a monopoly price or because the preferences of consumers were different.

Without knowing the praxeological relationship between past prices (as it is for instance in the case of prices for factors of production versus prices of the goods obtained by using them) we cannot say that, for instance, an increase in price is correlated necessarily with a diminution in consumers' welfare. That result may be, instead, the consequence of a more intense desire for this good. Behind these prices are different historical circumstances, as Mises explained:

> There are monetary units and there are measurable physical units of various economic goods and of many—but not of all—services bought and sold. But the exchange ratios which we have to deal with are permanently fluctuating. There is nothing constant and invariable in them. *They defy any attempt to measure them. They are not facts in the sense in which a physicist calls the establishment of the weight of a quantity of copper a fact. They are historical events, expressive of what happened once at a definite instant and under definite circumstances. The same numerical exchange ratio may appear again, but it is by no means certain whether this will really happen and, if it happens, the question is open whether this identical result was the outcome of preservation of the same circumstances or of a return to them rather than the outcome of the interplay of*

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> *a very different constellation of price-determining factors.* (Mises 1998a, p. 211; emphasis mine)

Even if the monopolist obtained in the past higher net proceeds by engaging in the so-called "restriction" of supply, he cannot infer from this that the conditions are the same today and consequently an additional restriction would once again increase his revenues. He has to judge future preferences of consumers and the price they will be ready to pay, every time he determines the quantity he will offer. His entrepreneurial understanding of the particular situation is not aided by theoretical knowledge. As there is no economic law that relates changes in prices to changes in net monetary proceeds, and as there cannot be, his entrepreneurial ability relies on the accuracy of his judgment and forecast.

There is yet another way to shed light on the concepts of "monopoly" and competitive prices, namely the counterfactual comparison. Mises's second condition for the emergence of monopoly prices expresses in fact a counterfactual comparison between the "monopoly price" and the competitive price (Mises 1998a, p. 278).[13] From this point of view, the relevant comparison is between the price as actually achieved on the market and the counterfactual exchange ratio which would have emerged if the demand, or supply, had been different. In our case, the restriction of supply by the producer is the cause of a (monopoly) price higher than otherwise. But in this case the method of trial and error cannot be used. Historical prices express only what they really are: past exchange ratios, open only to historical understanding. These past prices are not related to present prices. We have seen above that the elements used by Mises to support a counterfactual comparison between competitive prices and monopoly prices–the restriction of production and the elasticity of demand as a condition for higher net proceeds than otherwise–are not coherent. Moreover, even the simultaneous use of the counterfactual analysis and the method of trial and error does not suffice to distinguish theoretically "monopoly" prices from market prices.

Restriction of Production and Reservation Demand

We will now examine the foundations upon which the distinction between competitive prices and monopoly prices rests. We will argue first that the concept of restriction of production cannot constitute the differentiating element between the two kinds of prices. Second, and more important, Mises changes the standard of comparison for monopoly prices from the market price to

[13]Mises (1998a, p. 278). For the concept of counterfactual comparison see Hülsmann (2001).

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competitive, equilibrium price.[14] In examples, Mises again shifts the standard of comparison back to the market price.[15]

For Mises, the achievement of a higher price (monopoly price) than otherwise (competitive price) is the result of a monopolistic restriction of production. This comparative reduction of the supply explains, in the Misesian framework, the lower satisfaction of consumers, the comparative loss in their welfare.[16] But this "restriction of production" represents a phenomenon of the market, indistinguishable from the formation of market, or competitive, prices.[17] These prices are formed in what Mises calls "the plain state of rest," a description of the functioning of the market. In Chapter 14 "The Scope and Method of Catallactics," in the explanation of the concept "the plain state of rest," Mises does not assume that the entire quantity produced will be offered and sold on the market. It is possible that producers have a reservation demand; in fact, by demand Mises understands the total demand to hold as explicitly mentioned by Rothbard (Rothbard 1993, pp. 118-23).

> People keep on exchanging on the market until no further exchange is possible because no party expects any further improvement of its own conditions from a new act of exchange. The potential buyers consider the prices asked by the potential sellers unsatisfactory, and vice versa. No more transactions take place. A state of rest emerges . . . which we may call the

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[14]In the equilibrium framework of a perfect adjustment of production to consumers' demand, i.e., the final state of rest, a reduction in the production of a monopolized good will cause a lower present satisfaction of consumers. I am indebted to Jeffrey Herbener for this interpretation of the Misesian monopoly theory. However, the question remains: is it possible to distinguish between market prices and "monopoly" prices *in the real world*? What is the relevance of the comparison between equilibrium price and monopoly price? For an important reconsideration of the proper place and role of the equilibrium construct, see Hülsmann (2000).

Let us accept for a moment that the equilibrium represents the appropriate standard for an evaluation of the impact of monopoly prices upon consumers' welfare, or that it is possible to have a monopoly price in equilibrium. As property rights are respected, the Rothbardian welfare criterion of demonstrated preference does not help us to identify a reduction in consumers' welfare. Thus, we arrive at the same problem described in section two. The unscientific interpersonal and intertemporal comparisons of utility will still be applied to support the idea of a comparative decrease in welfare. Therefore, even if a consistent definition of monopoly price exists, we cannot deduce from the existence of monopoly price a decrease in consumers' welfare. Moreover, we cannot base an economic policy on this argument. I am indebted to Cristian Comanescu for the last point.

[15]See Mises (1998a, p. 369) where he uses the concept of "potential competitive market price."

[16]See Mises (1998a, pp. 381-82) for the idea that, irrespective of the particular reaction of the consumers to the higher price, the consumer's "satisfaction appears to be impaired from the point of view of his own valuations."

[17]Identical with nonmonopoly "market prices," Mises (1998a, p. 278). As has been stated above, the restriction of production is explained by allocation of scarce resources to more remunerative alternative uses.

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> *plain state of rest. . . . Only those potential sellers and buyers who consider the market price too low or too high respectively have not sold or bought.* . . . [T]he notion of the plain state of rest is not an imaginary construction but the adequate description of what happens again and again on every market. (Mises 1998a, p. 354: emphasis mine, except for "plain state of rest", emphasized in original)

Thus Mises seems to believe that competitive prices are compatible with a reservation demand, i.e., the withdrawal of a part of the supply from market for consumption, or speculation.[18] This means that the difference between competitive prices and "monopoly prices" is only a difference in the degree of reservation demand, not one of kind. Let us repeat here that this conclusion results from Mises's definitions. It follows that we cannot use the restriction of production as a criterion to differentiate between monopoly and competitive prices.

In the chapter dealing with monopoly prices, Mises abandons the above characterization of competitive prices, which does not allow him to differentiate monopoly from competitive price, because both types of prices are compatible with a reservation demand. He now uses a characterization of competitive prices that is closer to the concept of final state of rest, (Mises 1998a, p. 354) and distinguishes monopoly from competitive prices, now understood as equilibrium prices toward which the market tends. However, this equilibration process does not explain either why people exchange goods at a certain moment in time, or the exchange ratio. From the *ex ante* point of view of the participants to an exchange, the terms of exchange agreed upon is an equilibrium price, i.e., it is the best alternative under given conditions. The fact that *ex post* one of them, or even both, regrets the exchange, i.e., judges that he could have better used his resource, does not change this.

Thus, Mises changes the meaning of the expression "competitive price." In Mises (1998a, p. 278), the competitive price is the real market price, while for Mises (1998a, p. 354), it becomes the equilibrium (final state of rest) price. Similarly, in a later short restatement of his understanding of the functioning of the market, Mises presents the case of monopoly prices as an exception of the "dominant market tendency to adapt production as closely as possible to consumer wishes" (Mises 1994, pp. 151-58).[19] Here he does not describe the formation of market prices in the real world in which buyers and sellers fulfill their goals. The main characteristic is now the tendency "towards a situation in which supply and demand coincide. At that price . . . for which we shall use the better term *final price*–everyone who wants to buy can buy and everyone who wants to sell can sell" (Mises 1994, p. 153).

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[18]See Block (1977) for a detailed discussion.

[19]The same characterization of competitive market by stressing the equilibrium achieved in the long run is found in a previous paper written by Mises (1998b, pp. 1-28) from 1944 and published under the title "Monopoly Prices."

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See comparatively:

> Competitive prices are the outcome of a complete adjustment of the sellers to the demand of the consumers. Under the competitive price the whole supply available is sold, and the specific factors of production are employed to the extent permitted by the prices of the nonspecific complementary factors. No part of the supply available is *permanently* withheld from the market, and the marginal unit of specific factors of production employed does not yield any net proceed. (Mises 1998a, p. 354; emphasis mine)

Thus, as we have pointed out above, the fact that the potential supply is not permanently withdrawn from the market is without importance for the price determination at a given moment in time. The magnitude of the price would be the same if the quantity produced but not offered is destroyed or is maintained for future speculation. This emphasis on the "final price" at which all the quantity produced will be sold on the market does not fit in the overall Misesian framework, where efforts are made to explain real market prices, not final equilibrium prices.

CONCLUSION

Built on Rothbardian insights, our attempt to show the peculiarities of Misesian monopoly theory results in a few conclusions.

First of all, in Mises's theory of monopoly two of the three conditions for the emergence of monopoly prices belong to different realms of scientific inquiry. On the one hand, Mises points out the idea of a counterfactual comparison between competitive price and monopoly price; on the other hand, he stresses the importance of an empirical method to discover monopoly prices. The latter, even if it describes a true statement about market conditions, i.e., the entrepreneurs do not know (beforehand) the market demand curve, does not help us to identify the monopoly price on market.

Second, Mises erroneously founds his welfare arguments on value theory. His utilitarian endeavor to show that "consumers' sovereignty" is infringed by monopolistic restriction of production does not succeed. He based his arguments on nonscientific interpersonal and intertemporal comparisons of utility.

Third, Mises is not consistent in the use of a standard of comparison for "monopoly prices": on the one hand, the market prices are not distinguishable from "monopoly prices"; on the other hand, the transfer of the discussion to the equilibrium framework does not help us either, as we try to explain real market phenomena.

Thus, Mises's attempts to incorporate the neoclassical concept of monopoly price into the framework of the market process, as depicted by Austrians, do not succeed.

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Our inquiry supports the largely shared opinion among Austrian economists that monopoly price (at least in its present definition) does not exist on the free market; it appears only, and is logically identifiable, as a result of a privilege given by the State.

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REALISM: AUSTRIAN VS. NEOCLASSICAL ECONOMICS, REPLY TO CAPLAN

WALTER BLOCK

This Reply is part four of an ongoing debate that first began with Caplan (1999). Part two can be found in Block (1999), and part three in Caplan (2001)[1].

Overall, the debate concerns the issue of whether the Austrian or the neoclassical vision more closely approaches the truth in economics, with regard to such issues as methodology, indifference, envy, *verschtehen*, continuity, demonstrated preference, welfare economics, public goods, and cardinality.[2]

WALTER BLOCK holds the Harold E. Wirth Eminent Scholar Endowed Chair in Economics in the College of Business Administration at Loyola University, New Orleans.

[1]There is a parallel debate to this one over many of the same issues taking place between Caplan and Hülsmann. It consists of first, Caplan (1999), second, Hülsmann (1999), third Caplan (2001) and fourth, Hülsmann (2002). I shall for the most part be focusing on Caplan's (2001) criticisms of Block (1999), tending to leave those parts of Caplan (2001) that criticize Hülsmann (1999) for the latter author's response. Another part of this debate is Barnett and Block (unpublished ms.)

[2]It is interesting to note that Caplan (1999) appeared in *Southern Economic Journal.* Block (1999) and Hülsmann (1999) were originally submitted to the *SEJ*, as befits the usual practice of such rejoinders being featured in the journal in which the "first shot" was fired. Only when these two papers were rejected by that journal did they appear in the *Quarterly Journal of Austrian Economics.* Even more curious, Caplan (2001) was published not by the *SEJ*, but by the *QJAE.* The debate between Caplan on the one side and myself and Hülsmann on the other is of course between representatives of neoclassical and Austrian economics respectively. The *SEJ* is a vehicle for the former position and the *QJAE*, as its name implies, for the latter. What conclusions can be drawn from this state of affairs? One possibility is that the praxeological school is interested in, and willing to debate issues that separate the two perspectives and the neoclassical (at least as represented by *SEJ*) is not. Another is that from the mainstream point of view, a critique of Austrianism is barely worth mentioning, and certainly not worth continuing, hence the acceptance of Caplan (1999) in *SEJ*, but the rejection by this journal of Block (1999), Hülsmann (1999), and even of Caplan (2001).

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The present article is limited to replying to Section 2: "Probability," of Caplan (2002).[3]

Before beginning my substantive reply, which will be entirely critical, let me say at the outset that I very much appreciate Caplan's willingness to engage with Austrians in intellectual debate, and, even more, the quality of his commentary. All too often the response of the neoclassical economists, of which Caplan is a splendid example, is merely to ignore criticism emanating from praxeologists. As is only human nature, I vastly prefer the former to the latter. But I go further. Caplan is unique amongst mainstream members of the profession in that he is actually knowledgeable about that which he criticizes. Further, it is marvelous to be taken to task for what one has actually said, rather than for what is falsely attributed to an author. In Block (1999) I complimented Caplan (1999) by comparing him favorably to Nozick (1977),[4] as a critique of Austrianism extremely well informed about its subject matter. Nothing in Caplan (2002) has induced me to change my assessment in this regard; if anything, it is only strengthened.

PROBABILITY

For Caplan (2002, p. 1) "reading my critics convinces me that the issue of probability permeates every aspect of this discussion." For me, the last refuge of the neoclassical economist lies in probability, at least insofar as concerns the use of it being made by Caplan. For while this author may not realize it, he is using probability as a stick to beat up on the Austrian concept of the synthetic *a priori*–without directly confronting it. He states (2002, p. 2; emphasis added), "On topic after topic, Block and Hülsmann make extreme claims about."[5]

I can see why a non-Austrian would interpret matters this way, but this is not precisely correct. As a "friendly amendment" to his statement, I would substitute "On topic after topic, Block and Hülsmann make apodictic, logically necessary, or synthetic *a priori* claims about."[6]

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[3]For a rejoinder to Caplan (2001) in its entirety, see Barnett (unpublished ms.) and Block (unpublished ms.).

[4]For a rejoinder to that article, see Block (1980).

[5]This sentence ends: "about how little people are able to know." This, in my opinion, is relatively unimportant. The real issue between Caplan and myself does not revolve upon what we claim people are able to know, a lot or a little. Rather, the debate focuses on the certainty, or the probability, on the basis of which this knowledge is held.

[6]This emphasis on the apodictic nature of Misesian (1957, 1998) economics should not be interpreted as some sort of grandiosity, or immodesty. It is not at all the case that praxeologists cannot make mistakes. There are errors of logic, errors of fact, errors of understanding, etc. Apodictic argumentation is no more free of the possibility of error than is mathematical or geometrical reasoning. Friedman (1953) claims that if two practitioners of apodictic discourse disagree, since they have no empirical experiments to help

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There is no way that all the talk in the world about "probability" can breach this fortress indirectly, as Caplan attempts to do. If it is to be overturned, it must be done directly, and this neoclassicists, particularly the best of them such as Caplan himself, are congenitally unable to do, since, as I shall attempt to show, to the extent they are competent economists at all of any stripe or variety, they must themselves adhere to these very "extreme" principles.

For an audience of praxeologists, the main readers of a journal such as *QJAE*, there is little more that need be said. But for the sake of those unschooled in this tradition, I need to elaborate[7].

What, then, is a synthetic *a priori* statement?[8] This can best be explained with the aid of Table 1. The column headings indicate ways of learning about the world. We learn *a posteriori* through experience. In contrast, we learn *a priori* through understanding; that is, as soon as we grasp the meanings of the words involved, we immediately seize upon the truth (or falsity) of a statement comprised of them. The row headings indicate the status of sentences; e.g., their "realism," or agreement with physical reality about which Caplan is so concerned. Analytic statements are true by definition, and have no application to the real world; they indicate merely how we choose to use words. For example, bachelors are unmarried men; or "elephants" are big gray creatures with tusks and a funny looking nose; or the bischon frise is a dog. Synthetic claims are specifically about the real world; e.g., I am now wearing a red shirt, water runs downhill. There is no debate concerning the status of three of the four boxes found in this two by two matrix. That is, no one doubts that there is such a thing as A, B, or C can exist.

Table 1

	A posteriori	*A priori*
Analytic	*A*	*B*
Synthetic	*C*	*D*

Take *A* first. Although there is some question about this in philosophical circles,[9] one way to characterize the elements in this set is in terms of the ways

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them determine who is right, they can only fight, or at best fail to come to any conclusion. But this is precisely the same position occupied by two mathematicians who disagree with one another, or two different theorists of geometry. Needless to say, no such conclusion applies in either of those cases, nor would anyone assert they would. Why only for praxeology?

[7]For further reading on these matters, see Hoppe (1988, 1992, 1997), Rothbard (1957, 1976, 1993, 1997a, 1997b), Mises (1957, 1999), Selgin (1988), Fox (1992), and Rizzo (1979).

[8]In preparing this section, I had the help of William H. Friedman, Michael Levin, and Jan Narveson. The usual caveats apply.

[9]The objection can be put as follows: only an ignoramus would even try to verify an analytically true statement by observation. Someone could actually undertake a survey of the men in his city to determine whether any of its bachelors were married, and if he

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in which we come to learn language. For example, "We shall use language in such a way that 'rouge' in French will translate to 'red' in English." We learn this through experience, but to say "Rouge equals red" is an analytic statement. For *A*, the analytic *a posteriori*, we can learn each of the words in this sentence through experience (in an *a posteriori* manner), and yet the meaning of it is tautological.

Now consider *B*. An example is "Bachelors are unmarried men." Another is that the bischon frise is a dog. These analytic *a priori* sentences are necessarily true, once we know the meaning of each of these words; their denial involves one in a self-contradiction. But it says nothing about the real world. Once we grasp the meanings of the words, we understand the statement to be necessarily true, without any further resort to experience.

Likewise, *C* is also unobjectionable: People can learn I am now writing on this paper and wearing a red shirt by observation. Or that water flows downhill. Or that dogs like to chase squirrels. Statements in this category fall in to the scientific realm of empirical studies, and also into the common sense world of observation.[10]

It is only *D*, the synthetic *a priori*, which presents any real controversy. For the claim, here, is that the statement is logically undeniable, and yet does not arise (only) from experience. Not being based on empirical observation, articulations of this sort cannot be *refuted* by appeal to evidence; they are true apodictically, by their very nature, and yet refer, and intimately so, to the real world. Consider some examples of noneconomic statements that are logically necessary, cannot be denied except upon pain of self-contradiction, and yet explain and help us understand the workings of the universe:

- Nothing is uniformly red and uniformly green all over its surface;
- Everything is getting older;
- The angles of a (Euclidian) triangle sum to 180 degrees;
- The Pythagorean theorem: $a^2 + b^2 = c^2$, where a and b are the sides of a rectangular-triangle, and c is the hypotenuse;
- I now exist.

These are very general statements about the universe. Nevertheless it is impossible to even imagine them to be false. Also, it cannot be denied that they are about the real world, and crucial to our understanding of it.

Now take a few cases from the realm of economics:

- Rent control, *ceteris paribus*, leads to less investment in residential rental housing;

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couldn't find any that were then perhaps he could be said to know an analytic truth *a posteriori*. But the question would be: how did he identify the bachelors in the first place without reference to their marital status? I owe this point to Michael Levin.

[10]Http://www.utm.edu/research/iep/a/a priori.htm.

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- The minimum wage law causes unemployment of unskilled workers, other things equal;
- There are mutual benefits from trade in the *ex ante* sense;
- Man acts (to create a world more to his liking in the future than the one which would arise but for his action);
- Demand curves are downward sloping (e.g., when forced to give up one unit of a stock of a good, the economic actor will give up the least valuable of them).

With regard to the contention over the status of the synthetic *a priori*, there are two schools of thought. One, the Austrian, based primarily on the writings of Mises, Rothbard, and Hoppe, accepts this insight, and not only with alacrity, but places it at the very basis of economic science. The other, the Vienna School, or logical positivists,[11] from which all neoclassical economists such as Caplan intellectually descend, rejects it, and with equal fervor.[12]

The view of the latter perspective may be summarized by the following, call it *E*: "A claim *either* applies to the real world, in which case it is at least in principle falsifiable on the basis of evidence, *or* if it is not falsifiable on the basis of evidence, then it *cannot* be applicable to the real world." But this perspective is internally self-contradictory.[13] The problem is, where does *E* fit into the categories depicted in Table 1? How can the statement, *E*, itself, be reconciled with its own substance? If *E* is merely an empirical claim, then it can only be known on a probabilistic or tentative basis; and if so, why all the certainty with which it is maintained by mainstream economists? What happened to the "probability" about which Caplan places such great reliance? On the other hand, if *E* is necessarily true, according to those who articulate it, then it *cannot* apply to the real world, and must be dismissed as merely giving us conventions about word usage. Either way, mainstream methodology is embedded in quicksand.

Caplan attempts to short-circuit these logical positivist difficulties with his emphasis on "probability." He cannot be allowed to get away with such a ploy. This way of looking at the world is fatally mired in the tired shop worn dogma of the Vienna School. My claim is that even Caplan himself cannot consistently maintain his own attempt to evade the primordial fact of the synthetic *a priori*. Consider two cases in point.

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[11]Schlick (1985), Carnap (1967), Friedman (1953), Harris and Severens (1970), Pap (1958), Quine (1980), Baillie (1996), Dummett (1996), Stroll (2000).

[12]Caplan (2001, n. 1) is very insightful in distinguishing Misesian Austrians (e.g., quintessentially Mises, Rothbard, and I would add Hoppe) from the non-Misesian ones, and of placing Hülsmann and myself in the former camp. In fact, based upon this one single crucial distinction, there is serious question as to the application of the honorific "Austrian" for the so-called non-Misesian members of this school.

[13]See Hoppe (1988, 1992, 1997).

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First, the recent brou-ha-ha about the minimum wage law, created by the publication of Card and Krueger (1994). These two worthies purport to show that contrary to most empirical findings of economists, a raise in the minimum wage, at least insofar as Pennsylvania and New Jersey at the close of the twentieth century were concerned, does not lead to unemployment of unskilled workers but instead to more jobs for them at higher wages. Now if mainstream economists were to have followed their self-announced positivistic methodology, they would have reacted to this "finding" in terms such as these: "Oh, well, isn't it interesting; economic law works differently in Pennsylvania and New Jersey at the close of the twentieth century," or "Oh, well, isn't it interesting; economic law works not at all in Pennsylvania and New Jersey at the close of the twentieth century," or, to utilize Caplan's probabilistic language,

> It is only *probable* that economic law would work in the same manner in Pennsylvania and New Jersey at the close of the 20th century as it does everywhere else and at all other times; Card and Krueger (CK) have shown, however, that what is merely probable did not work out as expected at least in this one instance.

But this is not at all the reaction of many of the best economist critics of CK. Instead, they came out swinging, with real fire in their eyes.[14] They cast aspersions on Card and Krueger's sampling technique, on their empirical work, on their calculations, on just about every aspect of their study. Hell or high water, they were determined to show the error of their ways to CK. In other words, to use Caplanian language, they made "extreme" claims. Nor was this merely a disinterested attempt on the part of these economists to check and see if CK were right or wrong; a fact-finding mission, so to speak, let the chips fall where they may. No. They *knew* that CK were in error, grave error, and they were going to prove it.

I asked Caplan if he had ever written on this topic,[15] and he said that while he had not formally published anything on the minimum wage, there was some material on it at his web page,[16] to which he referred me.

Let us consider some passages from Caplan's classroom teaching notes.[17] Note the definitiveness of Caplan's views on the minimum wage. No "probabilistic" talk here. Specifically, he states:

- At the equilibrium wage, there are neither labor shortages nor surpluses;

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[14]Becker (1995), Burkhauser, Couch, and Wittenburg (1996), Deere, Murphy, and Welch (1995), Gallaway and Adie (1995), Neumark and Wascher (1992).

[15]Via email, in June 2001.

[16]Http://www.gmu.edu/departments/economics/bcaplan/e321/lab2.htm.

[17]Apart from the fact that they are extremely informative, and that I wish I were even half as well organized in my teaching as he.

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- unemployment is *voluntary*;
- how is *involuntary* unemployment possible? *Only if the prevailing wage is too high!*
- This is no different from any other surplus good;
- There is no case where workers are *both* "underworked" and "underpaid";
- The general solution to all involuntary unemployment boils down to: reduce the market wage until the surplus disappears;
- Suppose the equilibrium wage is $10/hr. If the government imposes a minimum wage of $15/hr., there will be unemployment;
- Simple question for proponents: Why not $1,000,000/hour?

One possible explanation for the unambiguous[18] nature of his language is that this is an undergraduate course, and it doesn't do too well to introduce all sorts of qualifications[19] in an introductory treatment of the subject. But I do not think this fully accounts for his stance on these issues. Query for Caplan: suppose one of his students took him up on the minimum wage of $1million/hour. Would he say it is very *likely* this would cause unemployment, or that it is undeniable that it would? I very strongly suspect the latter.[20]

Caplan's probabilistic methodology is incompatible with his strong support of free enterprise. There are two different perspectives on the latter, call them principled and empirically based. The former favors free markets as a matter of principle, apart from empirical findings; the latter favors free markets if and only when empirical findings support them. As an economist, Caplan happily seems to belong to the former. As a methodologist, unfortunately, to the latter.

Let me give another example from my own early professional life. I was a graduate student at Columbia University in the late 1960s, working on my Ph.D. dissertation on rent control. I was attempting to empirically demonstrate that this law causally brings about less investment in rental house building, greater vacancies, reduced labor mobility, etc. Most of the time, my econometric results yielded the correct sign for the independent variables (e.g.,

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[18]The one possible counterexample to my claim is Caplan's statement: "This simple application of S&D runs contrary to almost all popular beliefs about labor. But there can be little doubt that it is correct." My interpretation of "little doubt" in this context, however, is that this is a rhetorical flourish, not a serious attempt to bring probability into the analysis.

[19]For example, he does not mention and then demolish the monopsonistic argument in favor of minimum wages, which I am sure he would do in a graduate course. For a critique of this argument, see Block and Barnett (unpublished ms.).

[20]I certainly hope that the latter is true; in my view, Caplan's very status as an economist rides on this issue. I have a moral certainty that were this gifted economist asked about this issue in any other context, he would come through with flying colors, as an "extremist."

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indicating the correctness of the usual economic analysis of this legislation). But every once in a while I generated the "wrong" signs; e.g., my equations zigged when they should have zagged. Did my faculty advisors congratulate me on these results? Did they brag about the fact that their young genius student Block was going to turn the world of economics upside down with his great and wondrous findings? That rent control, *mirible dictu*, could actually help poor tenants? Not a bit of it. Instead, they said something to the effect of: "Block, you dummy, get the hell out of my office and go run these regressions again until you get them right." But if the basic supply and demand analysis of rental housing is only "probable," then my (erroneous) results should have had as much standing as those of anyone else, including my own "correct" findings. My faculty advisors were good economists, happily, otherwise they would have accepted my erroneous findings without demur. Or, at the very worst, tolerated them as correct, but "improbable." To say the least, that was not their reaction to them at all.

How does Caplan stand on rent control? Again, quoting from his web page,[21] he gives the standard analysis:

C. The short-run effect: Admittedly, the physical stock of housing doesn't decline much, but quantity demanded rises substantially. The result: rationing.

D. Standard form of rationing: landlords retain their pick of tenants, but tenants get to stay as long as they keep paying the regulated rent.

E. The long-run effect: Housing supply gets much more elastic. New construction falls off, as does maintainence (sic). Shortage gets more and more aggravated over time.

F. Hidden margins: While the physical housing stock takes time to deteriorate, there are numerous ways for landlords to respond more rapidly. General strategy: cut quality. With a shortage of housing, you will still be able to get tenants.

G. Examples:

   1. Eliminate free services.
   2. Charge extra for furniture.
   3. Cut back on safety and maintainence (sic).

[21]Http://www.gmu.edu/departments/economics/bcaplan/e103/micro5.htm.

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Again, we find no probabilistic stipulations. Rather, in a straightforward manner, he depicts the standard analysis of rent control. Thus we see that Caplan, too, since he is a highly competent economist, approaches issues such as the minimum wage and rent control in precisely this synthetic *a priori* manner. But if so, then this would undermine the positivistic claims made in the section of his paper on probability.

One related instance arises in Caplan's (2001, p. 4) unwarranted attack on Mises for maintaining that there *are* "exceptionless causal laws." But there are "exceptionless causal laws" in economics: every synthetic *a priori* statement can serve as an instance. Caplan believes, in contrast, that "We merely require beliefs about conditional probabilities." But is it only "conditionally probable" that minimum wage laws unemploy workers with marginal revenue productivity below the levels mandated in such legislation? That rent control leads to housing disarray? That trade is mutually beneficial in the *ex ante* sense? Even Caplan himself does not really believe this, let alone numerous other neoclassicals who have not allowed their mistaken methodology to get in the way of their more accurate economic practice. Caplan refuses to see economics in any other way than merely probable. As methodologist, he refuses to acknowledge economic law; but as economist, as we have seen, he transcends this.

Let me put this point on probability in an entirely different way. Consider Table 2.

Table 2

	Low probability	High Probability
C (empirical statements)	I	II
D (synthetic *a prioris*)	III	IV

Caplan maintains that all is probability in economics. He taxes Hülsmann (1999) and myself (Block 1999) with ignoring this "insight." Well and good. Let us take him at his word. With the aid of Table 2, we can accommodate his concerns within the Austrian nexus. The columns indicate high and low probability, the rows distinguish between empirical statements and synthetic *a priori* ones. Caplan wants to separate the columns, or to stress the differences lying in this dimension,[22] at the expense of that regarding the rows. The Austrians, in contrast, typically place much more emphasis on the latter than the former, but are perfectly able to accommodate both.

[22]As depicted, probability admits of only two levels: high and low, for ease of exposition. This could easily be changed into a continuous variable, starting with zero probability on the left and ending with 100 percent certainty on the right.

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Let us furnish some examples so as to flesh out this table.

1. An example of a low probability empirical statement is that all elephants wear red suspenders or that the price elasticity of demand for bananas in New Orleans in 1991 was exactly minus two.

2. A high probability empirical statement is that elephants are very heavy, and that the price elasticity of demand for bananas in New Orleans in 1991 was somewhere between minus .001 and minus 100.

It is easy to see how we can incorporate probability into empirical statements: they are intrinsically probabilistic in the first place. Doing so for synthetic *a prioris* will be a bit more of a challenge. But this is only because they have been mischaracterized as being certain.[23] This is not so. Rather, a synthetic *a priori* statement is of a different logical type than an empirical one.

3. and 4. A relatively highly complex and therefore low probability synthetic *a priori* statement is the pythagorean theorem, to the effect that the sum of the squares of the sides of a right triangle equal the square of the hypotenuse. A higher probability and less complex statement is that all triangles have 180 degrees. An even simpler and thus yet higher probability one is that all triangles have three sides. Why the variation in probability? It is because the earlier the sentence of these three, the more steps that must be taken in the logical proof, and the more (and more complicated) the steps, the greater the opportunity for human error; the later, the simpler, and the fewer steps in the argument. We might misunderstand words. We might make an erroneous deduction from one step in the argument to another. The more elementary, short, simple and basic the statement, the less likelihood of such error.

Now consider some economic postulates, all of them synthethic *a priori*, and therefore necessarily true on the assumption we understand and deduce correctly. Some simple high probability short statements are: I exist; I act. Some intermediate level ones are: minimum wages lead to unemployment; rent controls promote housing shortages; and trade is mutually beneficial in the *ex ante* sense. An example of a complex, many stepped (and therefore in the terminology we are currently employing) "less probable" but still apodictic argument is the Austrian business cycle theory or its money regression theorem.[24]

The problem I have with Caplan's (2001) remarks on probability is that he uses them to in effect deny the existence of the rows. But this need not be the

[23]Praxeologists have been interpreted by critics as intellectually arrogant. For a reply to this charge, see Hoppe (1998).

[24]Lest I be misunderstood, I am saying with regard to the Pythagorean theorem, and Austrian business cycle theory, etc., that since they are comprised of relatively long chains of reasoning, there is more room for human or psychological error in deducing their conclusions. They are thus only less probable in this very restricted sense. I am not saying that they are any less apodictic than less complex synthetic *a priori* chains of reasoning.

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case at all. That is, just because there is a valid distinction to be made between high and low probability, on the one had, it does not mean that there is none to be drawn between claims about the world based upon empirical evidence and those based on apodictic reasoning, on the other.

To Caplan on probability I offer the following rejoinder: Say if you will that probability is all-pervasive; that nothing exists without probability. But this will avail you nothing in your attempt to obliterate the primordial distinction between an empirical claim and one that attains the status of the apodictic. For synthetic *a priori* statements, too, if only in the sense mentioned above, can partake of degrees of probability.

Moreover, if it is only "probable" that the minimum wage causes unemployment, and that rent control leads to housing shortages, then when these laws are enacted, *sometimes* they have these effects, and *sometimes* they do not. But this hardly accounts for the reaction of the mainstream economist to Card and Krueger's "discovery" that the minimum wage had no such deleterious effects in their "study." It hardly accounts for the reaction of my dissertation advisors to my detection of the opposite from expected effects of rent control. It hardly accounts for Caplan's own nonprobabilistic language concerning these attempts to fix prices on his own website. No, Caplan cannot have it both ways. If he wishes to stick to his story on probability, then he must jettison his certainty about shortages and surpluses in supply and demand (and much, much more in economics). If he wishes to continue amongst the ranks of economists who see clearly on such subjects, he must renounce his methodological views on probability.

Caplan (2001, p. 2, n. 2) upbraids me for mentioning slightly inaccurate roulette wheels: "if people can keep using class probabilities in spite of 'slight' unique aspects, what prevents them from using them (with less and less success) as the unique aspects expand indefinitely? Moreover, calling a *deviation* 'slight' is itself a quantitative judgment about a unique event!"

First, these are not unique events; rather, the opposite.[25] Second, and more important, Caplan confuses a difference in degree with a difference in kind. Class probability involves endlessly repeatable non-unique events, such as games of chance, cards, dice, coins, roulette, bingo, etc. With regard to them, we do have probability distributions (we can precisely quantify the likelihood of a seven or snake eyes in dice games, or of five heads in a row in coin flipping). Case probability concerns nonrepeatable unique events, about which we have no information about probability distributions. Indeed, such a concept, when applied to such things as elections, or sales of a good, is logically incoherent.

[25] I am here implicitly abstracting from phenomena that render this false; e.g., every time the wheel spins, the temperature in the room, and the air pressure, are slightly different.

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Now it is true that dice tosses are not *exactly* "endlessly repeatable non-unique events." There is temperature, air pressure, the fact that if the dice are used thousands of times there might be differential wear on their sides, particularly if they were not perfectly balanced at the outset. There is always the possibility of cheating. On the other hand, surely we have some information about future presidential elections or sales.

Because of this lack of perfection in the distinction between these two types of probability, Caplan wishes to deny that there is difference in kind to be drawn. Not so. This can be shown by the fact that it is at least possible to perceive a state of the world where dice and coins are balanced perfectly. If so, then we can deduce probability distributions with mathematical precision. But due to that little inconvenient matter of free will, it is not possible to even contemplate any such analogous situation when confronted with issues based on human choice.

Caplan (2002, p. 6) calls into question Hoppe's (1997, p. 56) claim that it is impossible "to give an exhaustive classification of all possible actions." He offers a "simple reply: one can always complete an enumeration by adding a catch-all 'other' category." But this goes far too quickly. The "other" category does not at all satisfy Hoppe's proper demand for a full enumeration. Rather, it is merely a name for our ignorance. It does not at all contribute to the exhaustiveness, the sum total, or our knowledge. Instead, it concedes our ignorance.

Caplan (2002, p. 8) condemns my statement (Block 1999, p. 30) "the emergence of new ideas necessarily overthrows all previous patterns of behavior." With great forbearance, he castigates this as a "wild exaggeration." I thank him for pointing out this error of mine, but I think it only just to go further than he in his condemnation of me. I do not regard this statement as it appears merely as a "wild exaggeration." It seems to me that this is no less than lunacy.[26]

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[26]Where oh where was Caplan when I needed a copy editor for Block 1999? I apologize for this typographical error.

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PROBABILITY AND THE SYNTHETIC *A PRIORI*: A REPLY TO BLOCK

BRYAN CAPLAN

Walter Block's (2003) supposedly "entirely critical" (p. 48) reply greatly narrows the gap between our positions (Caplan 2001, 1999, 1997; Block 1999). It is a major concession for Block to admit that synthetic *a priori* propositions can have a low probability and empirical propositions can have a high probability. The implication, though he does not admit it, is that his initial claims most contrary to common sense are mistaken.

Furthermore, on the general topic of the synthetic *a priori*, Block incorrectly classifies me as a logical positivist. At no point in my critique did I fault Austrians for their belief in the synthetic *a priori*, or dismiss any of their positions for failing to be "verifiable" or "falsifiable." My position is not that their key arguments are methodologically flawed, but that they are mistaken. It may surprise Block, but I fully recognize the existence of synthetic *a priori* truths. However, most of his economic examples do not qualify.

Block's misunderstanding of my methodological position is partially understandable. Though his stereotype does not fit me, it is grounded in fact. Neoclassical methodologists have long subscribed to some version of logical positivism that working economists routinely flout. But there is no need, I shall argue, for neoclassicals to become praxeologists to eliminate the contradiction. All they need to do is jettison logical positivism and become thoroughgoing Bayesians (Howson and Urbach 1989).

My reply is organized as follows. The second section shows that if synthetic *a priori* claims, like empirical ones, vary in probability, then Block has no reason to single out the latter as unscientific. Section three sketches my position on the synthetic *a priori*, and explains the value of empirical testing

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BRYAN CAPLAN is an associate professor of economics at George Mason University. I would like to thank Peter Boettke, Ed Stringham, Alex Tabarrok, and Tyler Cowen for numerous discussions on these topics, Joseph Salerno for the invitation to respond, and the generous support of the Mercatus Center. The standard disclaimer applies.

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of less-than-certain *a priori* propositions. Section four provides a nonpraxeological solution for the methodological schizophrenia that Block correctly diagnoses in mainstream economics. Section five concludes.

Are High-Probability Empirical Claims "Scientific"?

Towards the end of his reply, Block provides the reader with a useful table. Its rows distinguish between empirical and synthetic *a priori* statements, its columns between low and high probability statements (p. 55; table 2). He then gives examples of all four logical possibilities; for Block, none of the categories are empty. There are high-probability empirical claims, such as "Elephants are very heavy," and low-probability synthetic *a priori* claims, like the Pythagorean Theorem. How can synthetic *a priori* propositions vary in probability? As Block explains: "the more (and more complicated) the steps [in a logical proof], the greater the opportunity for human error"[1] (p. 55; table 2). For example, it is less probable that the minimum wage leads to unemployment than that I now exist. (Table 2 appears in Block's article in the current issue of this journal.)

Block's table is eminently sensible. Unfortunately, it pulls the rug out from under the methodological position of his earlier reply. In particular, consider the "extreme" claims for which I criticized Block (Caplan 2001, p. 69), such as:

1. The envious "have no way to *prove* they lose out at all."

2. "Scientifically we can know *nothing* about states of mind not demonstrated in action."

3. "How can we, as economists, even know they [states of mind not demonstrated in action] exist?"

Where do claims about states of mind fit in Block's typology? At least a fair number must fall into category II—high-probability empirical claims. Take for example "Most socialists would be offended by *Defending the Undefendable*." It is hardly true *a priori*, yet it is plainly true. How then can Block label such a claim scientifically empty? Indeed, how can he call it scientifically *inferior* to a low-probability synthetic *a priori* claim? His own typology offers no basis for doing so.

[1] A corollary, which Block does not explicitly state, is that measures to reduce the opportunity for human error in a proof raise its probability. Even a complex theorem becomes virtually certain if thousands of mathematicians have checked and rechecked it for thousands of years. For this reason, the Pythagorean Theorem is a poor example of a low-probability synthetic *a priori* claim. For the sake of convenience, however, I will stick with Block's chosen example.

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One can only guess at the rationale Block would offer. He has already foreclosed one route: equating "scientific knowledge" with certainty. After all, he admits that many synthetic *a priori* claims–like empirical ones–fall short of certainty. If "Most socialists would be offended by *Defending the Undefendable*" is unscientific *due to lack of certainty*, so are claims about the minimum wage.

Might Block instead equate "scientific knowledge" with "synthetic *a priori* knowledge"? This would be a peculiar position. No one dubs physics and chemistry "unscientific" due to their empirical character. Why should common-sense claims about people's preferences be held to a higher standard?

To put my point in context: The reason I originally introduced probability was not "as a stick to beat up on the Austrian concept of the synthetic *a priori*." (Block 2003, p. 48) My goal, rather, was to use probability as a stick to *ward off* Austrians' unwarranted dismissal of common-sense empirical claims about human preferences, indifference, and the like. By admitting that the empirical can be more probable than the synthetic *a priori*, my critic has distanced himself from the weakest parts of his first reply.

A Common-Sense Approach to the Synthetic *A Priori*

Many of Block's examples of the synthetic *a priori* are poorly chosen. Demand curves, as previously explained, do not necessarily slope downward. Rothbard's own discussion of backward-bending curves admits as much. Nor is it necessarily true that raising the minimum wage reduces employment, *ceteris paribus*. It does not have to do so under monopsony.[2] Or to take a more fanciful counterexample, imagine that employers have been hypnotized to believe that the minimum wage declines every time Congress increases it. Then raising the minimum wage would increase employment instead of decreasing it, at least until bankruptcy set in.

In spite of these objections, and contrary to my critic's suspicions, I readily admit that synthetic *a priori* knowledge exists. I have no complaints about Block's noneconomic examples (p. 49). My objections to "There are mutual benefits to trade in the *ex ante* sense," and "Man acts (to create a world more to his liking in the future than one which would arise but for his action)," are only quibbles. To be strictly true, these assertions would have to allow for indifference: "There are mutual nonlosses to trade in the *ex ante* sense," and "Man acts (to create a world as or more to his liking in the future than one which would arise but for his action)." My difference with the Austrians, then, is not on the general question of synthetic *a priori* knowledge, but on

[2]Block is incorrect (p. 53, n. 19) to state that I would "demolish the monopsonistic argument" in a graduate class. I think that monopsony is a *bona fide* exception. This does not mean, of course, that raising the minimum wage always increases the quantity of labor a monopsonist employs, but it can under certain conditions.

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the particular question of what *qualifies* as synthetic *a priori*. My list is more selective, but it is not blank.

Still, if there are only a handful of *bona fide* synthetic *a priori* truths in economics, the relative value of empirical work increases. Moreover, once you admit, as Block does, that synthetic *a priori* claims vary in probability, then even the synthetic *a priori* can be empirically tested. Contradictory? Not at all. It is true, as Austrians have long insisted, that there is no point empirically testing synthetic *a priori* claims *known with certainty*. Empirically testing less solid synthetic *a priori* propositions, in contrast, can be fruitful.

Imagine that we were uncertain about the Pythagorean Theorem. We could spend more time checking the steps of the proof. On the other hand, we might measure a wide variety of triangles to verify that $c^2=a^2+b^2$. *Both efforts would rationally tend to increase our confidence in the theorem, even though neither is foolproof.* If we found a clear empirical counterexample, we would conclude that either the premises or the reasoning behind the theorem were incorrect, despite our inability to pinpoint them. Similarly, if we have doubts about Austrian business cycle theory, we might spend years reviewing the steps of the proof, or we might study economic history and look for counterexamples. If less-than-certain empirical evidence contradicts an absolutely certain synthetic *a priori* claim, then, as the Austrians advise, we should ignore the empirics. The opposite holds if indubitable empirics contradict a less-than-certain synthetic *a priori* claim.

A Bayesian Cure for Methodological Schizophrenia

When Block dissects my lecture notes on the minimum wage, he states: "Would he say it is very *likely* that this would cause unemployment, or that it is undeniable that it would? I very strongly suspect the latter" (p. 53). He suspects incorrectly. I have already stated that demand curves do not necessarily slope downward, so it would be illogical to proclaim that the disemployment effect of the minimum wage holds without exception. The reason for the unambiguous nature of my lecture notes is precisely the one Block prematurely rejects: They were written for an undergraduate class, where it is hard enough to communicate the main point, let alone any subtleties.

Why then, as Block insightfully asks, do economists react so violently to empirical evidence against the conventional view of the minimum wage's effect? Why did Block's dissertation advisor make him run regressions on rent control, then dismiss his anomalous results out of hand? There are three explanations worth considering:

> Explanation #1: Neoclassical economists are covert praxeologists, though they will not admit it.

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Explanation #2: Neoclassical economists are intellectually dishonest dogmatists posing as empirical scientists.

Explanation #3: Neoclassical economists are Bayesians with some strong priors.

Explanation #1 is essentially Block's: Neoclassical economists suffer from methodological schizophrenia. When they shut their eyes and close their ears to contrary empirical evidence on the minimum wage, neoclassicals are doing the epistemically correct thing even though it contradicts their official methodology.

Explanation #2 is routinely offered by economists who publicize their counter-intuitive empirical findings. To some extent, they have a point. There are more than a few economists who claim to be empiricists, but never change their minds about anything–even on questions that Mises himself would have seen as empirical.

For the most part, though, I prefer Explanation #3. The Bayesian model of belief formation permeates neoclassical economics. Since it will be unfamiliar to many Austrians, let me elaborate. The Bayesian model states, in essence, that rational people do not revise their whole worldview every time a new data point emerges. Rather, they marginally update their initial views as facts comes in–ideally according to Bayes' Rule, one of the most important formulas in probability theory (Varian 1992, pp. 190-2):

$$P(A|B) = \frac{P(B|A)^* P(A)}{P(B|A)^* P(A) + P(B)|\text{not } A)^* P(\text{not } A)}$$

P(A) is the "prior probability" of A–the degree of belief you assigned to A before seeing the evidence, B. P(A|B), read "the probability of A *given* B," is your "posterior probability"–the degree of belief you assign to A *after* seeing the evidence, B.[3]

Now consider the implications of Bayes' Rule for the Card-Krueger minimum-wage debate. Suppose, based on his pre-Card-Krueger knowledge, that Gary Becker's P(minimum wage reduces employment) was 98 percent. In Bayesian language, he had a "strong prior": he was almost, but not quite, sure of the standard conclusion. But recognizing the flaws of empirical research, he might have only weak expectations that any one study would confirm his view. Thus, Becker's P(Card-Krueger results| minimum wage reduces employment) is, say, 30 percent. Even if he is right about the minimum wage, there is still a 30 percent chance that a careful study by two Princeton economists will find the opposite. Becker would presumably have similar doubts about

[3]Note that the basis for both the prior probability and the subsequent evidence could be either synthetic *a priori* or empirical, though admittedly most applications emphasize the latter.

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P(Card-Krueger results| minimum wage does not reduce employment). Even if the conventional view were *wrong*, it is hardly certain that the Card-Krueger empirical test would expose it as such, so Becker assigns this conditional probability a mere 70 percent.

Given all this, what probability should Becker assign to his original view after reading Card and Krueger's paper? Using Bayes' Rule:

P(minimum wage reduces employment|the Card-Krueger results)

$$= \frac{.3*.98}{.3*.98 + .7*.02}$$

which simplifies to 95.5 percent. This is of course less than Becker's prior probability of 98 percent. The important fact, however, is how *little* his probability changes. He can rationally remain almost certain that the minimum wage reduces employment, although he must be slightly less certain than before. Given his initial certainty and the imperfections of the test, to change his mind more would be an overreaction.

Now consider Block's assertion that "[I]f the basic supply and demand analysis of rental housing is only 'probable,' then my (erroneous) results should have had as much standing as those of anyone else, including my own 'correct' findings" (p. 54). In a sense this is true—if you become more sure when you find a negative effect of rent control, you must become less sure when you find a positive effect. This does not imply, however, that both findings are *equally* reliable. Indeed, Bayes's Rule shows that we should automatically be more skeptical of empirical evidence in favor of an improbable conclusion.

How so? Compare the P(Block's econometrics were flawed| he found rent control causes shortages) to P(Block's econometrics were flawed| he did not find that rent control causes shortages). Suppose that P(Block's econometrics were flawed) is 30 percent, P(he found rent control causes shortages| Block's econometrics were flawed) is 45 percent, and P(he found rent control causes shortages| Block's econometrics were not flawed) is 80 percent. Then P(Block's econometrics were flawed| he found rent control causes shortages) is:

$$\frac{.45*.3}{.45*.3 + .8*.7} = 19.4\%$$

On the other hand, P(Block's econometrics were flawed| he did not find that rent control causes shortages) is:

$$\frac{.55*.3}{.55*.3 + .2*.7} = 54.1\%$$

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On these assumptions, Block's anomalous results were almost three times as likely to be flawed as his regular results. His dissertation committee justifiably greeted the former with extra skepticism.

The Bayesian position thus stakes out a compelling middle ground between atheoretical positivism and praxeology. On the one hand, the Bayesian view emphasizes that few propositions in economics are known with certainty, and that we should adjust our probabilities as new information comes in. On the other hand, the Bayesian view recognizes that the rational view is not an average of past empirical findings, much less a naive faith in the last prominent study.

Conclusion

Contrary to Block, the synthetic *a priori* has little to do with our dispute. My critique of the Austrians is not that their methods are "unscientific," but that their most distinctive positions are false or overstated. Yet Block's latest reply does inadvertently make Austrian economics more reasonable. If synthetic *a priori* claims vary in degree of probability, they can no longer be treated as scientifically superior to empirical claims. Furthermore, while empirically testing absolutely certain synthetic *a priori* claims is pointless, empirically testing uncertain synthetic *a priori* claims is not.

As is often the case with Austrians, Block is better at criticizing neoclassicals than he is at producing a sound alternative. He is right that most economists do not practice the logical positivism that they preach. He is also correct to maintain that logical positivism is mistaken. However, both of these problems can be resolved if neoclassical economists themselves adopt the Bayesian model of belief formation that they routinely apply to everyone else.

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REVIEW ESSAY

LIBERTY AND HARD CASES. BY TIBOR MACHAN. STANFORD, CALIF.: HOOVER INSTITUTION PRESS, 2002.

The case for liberty and the free market must be made on two fronts. On the one hand, a sound theoretical foundation supporting private property rights and limited government intervention is indispensable for such an endeavor. On the other hand, practical demonstrations of how such a system would function are also vitally important. No matter how compelling a theoretical argument against State action in the abstract may be, there remain the so-called "hard cases," which most people consider simply intractable absent government assistance, if they can even conceive of such an alternative approach at all. One commonly hears that, e.g., health care, defense, or technology are areas where the State must play a central role to remedy alleged "market failures" or to provide "public goods" that individuals, freely associating by exchanging their own property, supposedly cannot.

One such area is the prevention and relief of natural disasters, the most prominent of which (as far as resulting damage is concerned) include floods, hurricanes, and earthquakes. Such events, due to their sheer physical awesomeness and the seeming capriciousness with which they strike, do much to reinforce the impression in many people's minds that there can be *no* other approach to dealing with disasters than to have the State intervene as necessary. Still, even for those of interventionist bent, some basic questions must be answered: how far *should* the State go in helping its subjects deal with disasters, both in their aftermath and in preparations against? What responsibilities do these subjects, both as individuals and as voluntary associations, have to bear? Can provisions against such events be viewed as similar to precautions taken against other, less destructive but still unpleasant or tragic, misfortunes? These issues are of particular importance to those generally opposed to government interventions, for unless one is prepared to abandon recourse to the State altogether, the question of how far one is willing to compromise one's adherence to liberty in the face of some emergency must be addressed.

These issues are the focus of *Liberty and Hard Cases*, a short collection of essays discussing the proper role of government and the private sector in disaster relief. The contributors are supportive of the notion that the free market, *in conjunction with a limited State*, can appropriately respond to such crises, and point out the numerous ways in which government, ostensibly the selfless protector of its afflicted citizens, has in fact exacerbated the very crises it is meant to alleviate. Most of these ways are probably familiar to free-market advocates. These counterproductive interventions include subsidization of disaster insurance premiums, offers of direct relief and loans to disaster victims to live in and rebuild in high-risk areas, and compulsion of both

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insurance companies to offer policies to such at-risk individuals, and of individuals not at risk to participate in such schemes. All of these public policies have the effect of making it *appear* to those living in, say, flood plains that they are in less danger (financial as well as physical) than they actually are. Such policies allow those people to bear less responsibility than they should, and hence to assume more risk than they otherwise would. At the very least, incentives to prepare for such disasters are weakened if not removed, and government-provided benefits have a "crowding-out" effect on private relief initiatives. (As an indirect effect, by forcing insurance companies to bear more risk than they otherwise would, these companies may either get out of the business in the high-risk region altogether [if they're not eventually bankrupted], or scale back coverage in those areas that they could beneficially serve, forcing *other* people to be exposed to greater dangers than they otherwise would be. As always, there is no free lunch.)

These are of course the classic moral hazard problems of any insurance contract, but with the State involved the ramifications are much greater. By "artificially" encouraging people to engage in high-risk behavior (such as living on beachfront property in a hurricane zone) or, even worse, allowing them to potentially *repeat* their calamity (by providing them with the means to rebuild their destroyed property in the same, disaster-prone area), the State increases the likelihood and magnitude of human and material suffering from a natural catastrophe. Only the State, with its taxation powers stemming from its status as a judicial and protective monopolist, can insulate itself from such actions, in a way that no private insurance company ever could.

The contributors to this volume further point out that the bureaucracies pledged to addressing these problems typically act like any other bureaucracy: that is, their primary interest becomes that of self-preservation and empire-building, not problem-solving, and their management of actual crises reflects this appetite for expansion (usually at the expense of smaller social or political units likewise engaged in reconstruction). Indeed, government at all levels, not just those agencies directly involved in response to some disaster, invariably exploit crises to increase their power over their subjects (as has been well-documented in Robert Higgs's *Crisis and Leviathan*, a classic work cited by several of the contributors). Such realities should also be of no surprise to free marketers, and it is a central theme of this volume that, regardless of how appropriate one may deem government disaster programs, one must be ever vigilant against the "slippery slope" of losing our liberties due to the urgency of some problem that allegedly requires increased State powers (that rarely return to the pre-crisis levels, what Higgs terms the "ratchet effect").[1] Indeed, a serious part of the danger is not just from the government agencies themselves, but from the acclimation the government's subjects come to feel toward State action, and the perceived normality and acceptance of such actions. Even if governments act with good intentions, they

[1]If anything, the contributors probably downplay the risks from agencies such as FEMA (Federal Emergency Management Agency). One does not have to be a right-wing conspiracy theorist to recognize that such agencies are at least as sinister as they are bumbling. Likewise, editor Machan's characterization of government interventions as "paternalistic" (p. xviii) may be apt in some circumstances, but the State's true nature may be a bit more unsavory than that. Furthermore, issues of corruption and patronage are, for the most part, only alluded to, but surely the logrolling and pork-barreling that accompany any sizeable disaster are enormous.

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always run the risk of undermining the culture of self-rule and responsibility that must be the basis for any free society.

These points are all well-taken, and the contributors do a good job of presenting and defending them. That is the strength of the volume, in making the case that with government assistance, no matter how welcome, there always comes a cost to the free society that may not be so desirable in the long term. (However, the authors tend to speak in terms of rather vague notions like "individual sovereignty" or "classical liberalism" rather than the more straightforward and justifiable concept of private property rights.) In discussing "what is wrong" and "what shouldn't be done," the book succeeds. In discussing "what is to be done," the book achieves mixed results (at least from an Austrian perspective), in this reviewer's opinion.

The first essay, "The Role of Government in Responding to Natural Catastrophes" by N. Scott Arnold, deals with the problem of insuring against disasters, and the impact that governments (mainly at the Federal level) have had on the private insurance industry in states such as Florida and California (where, respectively, the concern is with hurricanes and earthquakes). These moral-hazard aspects have already been brought up, and Arnold details the manner in which the political process has led to greater State regulation, mandated benefits, and the like. However, Arnold makes some questionable claims about the nature of insurance, and about the ability of the market to provide such services.

Specifically, he argues that the risks facing private insurance companies covering disasters are correlated, so that the occurrence of one claim against a company is usually associated with the occurrence of several other claims (thus threatening bankruptcy), and that "informational asymmetries" between insurer and insured prevent the realization of mutually beneficial transactions due to the fact that low-risk people will not buy insurance at the price it must be set to cover high-risk people (so that there is "market failure": insurance companies either cannot enter into policies with the low-risk individuals, or they must prudently limit their coverage of high-risk individuals who want such policies). As such, there is some limited role for government in rectifying these problems faced by the private sector. Given these claims, it would probably be good to briefly discuss the Austrian conception of insurance.

The mathematics of probability theory is frequently used as a way of quantifying risk and uncertainty. However, risk and uncertainty are not the same things. Ludwig von Mises (1998, chap. 6) distinguishes between two types of probability that acting man may resort to: class probability and case probability. Mises (p. 107) explains:

> Class probability means: We know or assume to know, with regard to the problem concerned, everything about the behavior of a whole class of events or phenomena; but about the actual singular events or phenomena we know nothing but that they are elements of this class.

In contrast, Mises (p. 110) holds that: "Case probability means: We know, with regard to a particular event, some of the factors which determine its outcome; but there are other determining factors about which we know nothing."

Mises categorically distinguishes these two types of probability. As an illustration of class probability, he uses the example of a lottery, where we *know* that a certain number of tickets will be winners, but we do not know *which* of the tickets will win.[2]

[2]See also Rothbard (1997), p. 229.

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This type of probability is amenable to mathematical methods, but *only* as a *representation* of something we *already* know (or assume to know); such methods add nothing to our knowledge of *particular* outcomes. If we had additional information about these, we would be able to form *different* classes of outcomes, with the same types of representations but again without any additional knowledge of outcomes regarding individual members of those classes (beyond the fact of their class membership). Case probability, however, is not amenable to any mathematical methods. Here one must rely crucially on one's understanding of the particular problem at hand to make a judgment about the likely outcome (see also Hoppe 1997).

In Mises's conception, risk is a form of class probability and can be quantified, whereas uncertainty is a form of case probability and cannot be quantified.[3] In particular, it is only risk, and not uncertainty, that can be insured against. To quote Mises (1998, pp. 108-09) again:

> Let us assume that ten tickets, each bearing the name of a different man, are put into a box. One ticket will be drawn, and the man whose name it bears will be liable to pay 100 dollars. Then an insurer can promise to the loser full indemnification if he is in a position to insure each of the ten for a premium of ten dollars. He will collect 100 dollars and will have to pay the same amount to one of the ten. But if he were to insure one only of them at a rate fixed by the calculus, he would embark not upon an insurance business, but upon gambling. He would substitute himself for the insured. He would collect ten dollars and would get the chance either of keeping it or of losing that ten dollars and ninety dollars more.

Furthermore, "[i]nsurance, whether conducted according to business principles or according to the principle of mutuality, requires the insurance of a whole class or what can reasonably be considered as such. Its basic idea is pooling and distribution of risks, not the calculus of probability" (p. 109).

This situation describes what commonly occurs with most health or auto insurance, for example. An insurer believes (to the best of his knowledge) that, out of a particular group of people, a certain number will become ill and incur some sort of financial obligation as a result. The insurer does not know *which* of the people will become sick, only that a certain number will. The insurer can accordingly charge premiums to offset these losses (plus charge for the service), and so in effect he bears no risk. If the insurer learns that, e.g., redheads become ill at a greater rate than non-redheads, he can reclassify the people in the insured group and charge different premiums based on different anticipated losses. However, the fact remains that the insurer is always making recourse to class, not case, probability. In essence, the insurer is making a judgment about the frequency and distribution of events, not people.

The situation does not prevail in the case of natural disasters, so it is not clear that insurance is the proper provision against such events. It is not the case, e.g., that a certain number of earthquakes (events) will occur in some region, causing a certain amount of damage such that premiums can be charged to *all* (or most) of the people in that region under the assumption that only a certain number of them will be afflicted (without knowing *who* those will be). Rather, the issue is that *one* event (the earthquake) might occur, but it is simply not known entirely *when* it will happen. Also, when the event occurs, it will affect all (or least a large number) of the people in

[3]Knight (1940) also makes this distinction.

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that region (but not outside it). In effect, there is no class of events and affected people. There is only a class of a single member, and the issue becomes one of judging whether a *particular* event will occur. That is, we are considering case, not class, probability. As such, it would not seem that insurance *can* be applied to such situations. In contrast, with ordinary insurance, we know (or assume to know) that the event in question *will* occur, just not *where* and *whom* it will affect.

The point is not that insurance companies do in fact take such considerations of class versus case probability into account, only that to the extent that they do not, they are not engaged in insuring against risk, but rather they are engaging in speculation regarding uncertainty. Consequently, the problem facing companies covering disaster insurance is not the fact that the risks are "correlated." Rather, the companies' problem is that they are in the position of Mises's insurer who only insures a single person; they are engaged in a speculation that may pay off most of the time, but could possibly lead to ruinous losses.

Thus Arnold's discussion of the difficulties faced by insurance companies, and the government policies that could alleviate these difficulties, seems beside the point. If insurance companies are, in essence, engaged in a form of speculation, then we should not expect such companies to be an appropriate means for people to provide against the obligations incurred by natural disasters. Unlike the case with ordinary insurance, *both* parties are in effect gambling. With ordinary insurance, only the insured is gambling; the insurer has completely accounted for the possible losses and charged appropriate premiums by assembling the insured into a suitable class. It is questionable whether such gambling is the best way for someone living, say, in Southern California, to prepare for the consequences of an earthquake, and it is equally questionable whether an insurance company which enters into such bets is to be viewed in a different light than, say, a bookie who is ruined when a major upset occurs in a sporting event. One may sympathize with such a person, but one does not typically recommend that social policy accommodate them.

What, then, *is* the appropriate provision against such calamities? In the essay "Dealing with Natural Disaster: Role of the Market," Barun S. Mitra makes a number of important points. To begin with, the fact that news media can quickly appear at the scene of a major catastrophe means that large numbers of people get to see up-close the immense physical damage and personal suffering that accompanies the event, and that it is easy for people to conclude that the situation is as bad as can be imagined. Yet, there can be no question that modern-day disasters cause much *less* death and destruction than comparable events of the past. This is especially true in the West, where the damage from such disasters is measured in dollars, not lives lost. Even in the Third World, however, such events kill far fewer people than they once did. Still, there is little doubt that the developed world weathers such storms better than the nondeveloped world, which of course leads one to ask why that is the case.

That this improvement has been accompanied by dramatic increases in the standard of living of ordinary citizens, and that disaster prevention and relief is far superior in the economically advanced West, is surely no coincidence. Mitra draws the obvious but seldom-stated conclusion from these facts: namely, that it is economic development, and the attendant advances in infrastructure, medicine, and capacity for charity that are the best tools for managing natural disasters. The equally obvious (but again rarely pointed out) conclusion is that the role of governments in addressing such disasters is to foster economic growth, which it can only do by respecting

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private property and the rule of law, cracking down on corruption (which invariably increases during relief efforts), and generally limiting its activities to law enforcement rather than legislating and regulating. Whether it is reasonable to expect *any* government to behave in such a way is a different matter. The main point is strong, however: it is capitalism, not government, that is needed to prepare for and reconstruct after a disaster.

The third essay, "A Fool's Errand" by John Ahrens, is in fact more of a defense of the limited State against anarchism (including, presumably, the anarcho-capitalism of Austrians such as Murray Rothbard) than it is an argument for some level of government management of natural disasters. By the author's own admission (p. 80), not much is concluded about such management except that, while some such intervention is necessary, it should be prudently limited. For the author, government-directed relief efforts are a legitimate function of the State, which for him exists to protect and secure those rights underlying a free society. Such rights are supposedly violated if some number of citizens (how many?) are permitted to languish as a result of some calamity (natural or otherwise, presumably). He invokes Hobbes's conception of man as a wolf, requiring another, stronger wolf (the State) to suppress and control those base instincts, and allowing cooperation and all its attendant benefits to flourish.

Yet surely this is backwards. To begin with, as Hoppe (2001) has pointed out, governments are composed of *men*, and even if we agree that man is in fact wolf-like, his nature is not changed simply by placing him in an organization and calling it "government." Such an "explanation" could not explain why governments, consisting always of a minority ruling over a majority, can persist. The answer to *this* paradox, as Hoppe (following Rothbard and de la Boétie) notes, is that States, however unpopular particular policies of theirs may be, are always considered legitimate by the ruled. In other words, if a significant number of people refuse to cooperate with the State (or indeed actively resist it), then that State will collapse, and there are many examples even in recent history of just this sort of phenomenon. Thus, cooperation must *precede* the establishment of any social organization (including the State) or, put differently, people must have some recognition of, acceptance of, and respect for property rights in order for cooperation between them to occur. People may have flawed ideas about what constitutes valid property rights (and these misunderstandings are precisely what the State exploits to remain in power), but that is a different issue. If the State is in fact an aggressor and a violator of (property) rights, then it can in no way be considered a protector of such rights, and any case on that basis for government involvement in disaster relief must be rejected. Governments can only assist victims of such disasters by appropriating the property of nonvictims (that is, by violating the rights of *others*).

In other words, governments do not exist to "protect our freedom and thereby facilitate cooperation" (p. 80). Even dictatorships must seek some sort of approval from their subjects, and people must *first* accept *some* form of cooperation as useful or just before they engage in it, such that dissenters from that order may be justifiably punished. Inasmuch as it is man's rationality that permits him to recognize the benefits from peaceful cooperation and specialization under the division of labor,[4] it is

[4]See Mises (1998, chap. 8) and Hülsmann (1999). Hülsmann (p. 716) quotes Mariam Thalos:

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also man's mistaken (but correctible) conceptions of justice that permits exploitative institutions like the State to flourish. However, just as man can be disabused of false notions, there is also no reason to suppose that natural disasters "may lead to an escalation of violations of individual rights: assault, fraud, looting" as Ahrens (p. 69) claims. Even if true, that does not justify the government coercing others into assisting the victims of such disasters, and Ahrens has not made the case that this is so. It is not even clear where Ahrens himself draws the line with such interventions, as he views (p. 72) the massive role of the State in welfare and medical care as not "particularly worrisome," and he is not terribly worried about prospective "relaxing [of] requirements of due process" during disaster relief efforts. Ultimately, his essay rests on the contradictory notion that in order to protect individual rights, government must violate individual rights.

The final essay, "Liberty, Policy, and Natural Disasters" by Aeon J. Skoble, is the best one in the collection. To begin with, he is the only contributor who recognizes (though only implicitly) Mises's distinction between case and class probability. Skoble writes (p. 98):

> Insurance companies base their premiums on the information they can gather about similar things, "similar" because although each case is different, one can extrapolate statistically from trends and probabilities. For example, health insurance premiums may be more expensive for a smoker, not because the insurance company knows that *this person* will incur greater medical expenses but because smokers tend to. Auto insurance premiums are higher if you own a late-model car and live in New York City, not because they know that *your car* will be stolen but because there is a demonstrable rate of thefts of certain models.

This superb passage aptly summarizes the issue at hand. And of course, Skoble's point that people who live in South Florida *should* expect to pay higher insurance premiums, and that myriad government programs have encouraged people to engage in riskier behavior than they otherwise would, is correct, although, as has been already argued, it is debatable whether such people should even expect that insurance *can* address the *uncertainty* (as contrasted with risk) they face.

Other parts of the essay deal very nicely with a number of issues already considered: moral hazard and erosion of individual responsibility, media proliferation and propaganda regarding government efficiency, and unwelcome escalations of government power during such emergencies. While his final recommendations for dealing with disasters (restricting agencies such as FEMA to coordinating rather than providing relief, and limiting government activities to "truly unexpected" events [i.e., residents of South Florida should not be terribly shocked that hurricanes strike their area, and should thus not be expectant of benefits obtained by the coercion of others]) consist largely of tinkering, he is fully aware of this limitation and leaves room for more "libertarian-oriented" solutions. In all, his essay is very nicely written and the most forcefully argued and philosophically sound in the book.

> [T]he existence of such a system [the State] presupposes prior collaboration, for how could such a system arise except as a result of very significant collaborative efforts? . . . Prior to anything resembling a legal system that can inflict penalties for breach of contract, what can serve the function of bringing potential collaborators together to erect such a system?

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Governments *always* attempt to exploit a crisis, and disasters provide a natural (no pun intended) excuse for them to do so. Their proffered "solutions" to problems (which they often create) invariably worsen those problems. As is the case with general standards of living, economic growth and development—in short, capitalism—has dramatically lessened the impact of Mother Nature. This collection of essays makes these points very clear, and is worth reading for that reason alone. In the end, however, the authors cannot really extricate themselves from the notion that the State simply *must* have some role to play in the mediation of such calamities, and so their case is weakened as a result. By assuming that the State (i.e., institutionalized compulsion) has at least some legitimate function in this case, prospective "privatized" solutions take the form of a square peg in a round hole. In such cases there can be no substitute for private charity efforts, motivated by a true concern for the members of one's group, and tempered by an understanding that it is each individual's responsibility to exercise prudent judgment and to be prepared to bear the consequences.

DAN MAHONEY
Atlanta, Georgia

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AMERICAN ECONOMIC POLICY IN THE 1990S. EDITED BY JEFFREY A. FRANKEL AND PETER R. ORSZAG. CAMBRIDGE, MASS.: MIT PRESS, 2002.

This book consists of 14 articles and comments about each by a three-member panel. The book's editors wrote one of the articles: "The Role of Institutions in the White House."

Many of those who contributed to this book participated in the making of economic policy during the 1990s. Alice M. Rivlin, for example, served as President Clinton's Director of the White House Office of Management and Budget before becoming Vice Chair of the Federal Reserve System's Board of Governors, and Robert Rubin was the Secretary of the Treasury in the Clinton administration. An example of an exception is panelist Murray Weidenbaum, who was President Reagan's first Chairman of the Council of Economic Advisers.

Other Washington veterans include N. Gregory Mankiw, Martin Feldstein, Alan Blinder, William A. Niskanen, David T. Ellwood, and Allan H. Meltzer. Like the book's editors, both of whom were members of Clinton's Council of Economic Advisers, most who served during the 1990s were members of the Clinton administration. Because this administration accounted for the great majority of that decade, this book is largely a review of the economic policy of the Clinton administration by a group of people with extensive credentials.

Among those who have not served in Washington is Joseph P. Newhouse, a professor of health care and management at Harvard University, who authored *Medicare* and commented on "Health Policy in the Clinton Era: Once Bitten, Twice Shy." Newhouse lauds the Clinton administration for bringing about through the Balanced Budget Act of 1997, an "unprecedented slowdown" in the rate of growth of Medicare spending that significantly contributed to this administration's "great economic achievement" of the first budget surpluses in decades.

Another author without a Washington background is Pamela Samuelson, Director of the Berkeley Center for Law and Technology, who co-authored with Hal R. Varian, "The 'New Economy' and Information Technology." Varian, who is the Dean of the School of Information Management and Systems at the University of California, Berkeley, is also no Washington veteran. In his comments about their paper, Paul Romer, a Senior Research Fellow at the Hoover Institution and professor of economics in the Graduate School of Business at Stanford University, pretty well summarizes

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this book when he says that "through a combination of good luck and good policy (pick your own weights), Clinton presided over a remarkably long and stable economic expansion."

As is to be expected, people who served in the Clinton administration sometimes disagree with those who served in a Republican administration. So it comes as no surprise that in this book Robert Rubin says that Martin Feldstein's claim that the strong economic performance of the 1990s was due to the Reagan administration's tax cuts in the 1980s "had roughly the same plausibility as attributing the performance to Herbert Hoover's policies."

Feldstein, who credits the Clinton administration with having made a "very positive contribution" to the development of Social Security reform, disagrees with Douglas W. Elmendorf, Jeffrey B. Liebman, and David W. Wilcox, who in their paper, "Fiscal Policy and Social Security Policy During the 1990s," conclude that tax increases and spending discipline played a significant role in bringing about an unexpected turnaround in the U.S. fiscal situation. (Good luck in the form of a strong economy, they say, was also important.) Feldstein contends that the primary explanation for the elimination of the budget deficit "was the increase in economic growth driven by the new technology that raised taxable personal incomes, corporate profits, and capital gains."

In his comments about "Fiscal Policy and Social Security Policy During the 1990s, Rubin advises against the "politically easy path" of substituting equities for debt because "having spent close to 28 years on Wall Street, I think that many people are not equipped, in terms of understanding securities valuation and in terms of discipline and thoughtfulness, to invest effectively in equity markets for the long term." He warns, too, that in periods of time when large numbers of people are "adversely affected in their individual portfolios, there is a substantial probability that the political pressure to make good on those losses from the federal budget would be enormous."

After comparing the performance of the economy during the 1990s with other recent decades in his paper, "U.S. Monetary Policy During the 1990s," Mankiw concludes that "Although the average levels of inflation, unemployment, and real growth were similar to those that were experienced in some previous decades, the stability of these measures is unparalleled in U.S. economic history." The fact that while the nation was experiencing "macroeconomic tranquility" the money supply was exhibiting high volatility, he says, discredits the monetarist view that "stability in the monetary aggregates is a prerequisite for economic stability." In large part, Mankiw claims, the stability of inflation, unemployment, and real growth was due to luck. He believes that the low rate of inflation experienced during the 1990s shows that "discretionary money policy can work well."

In his comments about Mankiw's paper, Blinder attributes, in part, what he believes to have been the successful monetary policy of the 1990s to the fact that, excluding himself, but including Rivlin, Clinton's "appointments to the Fed were truly excellent–and quite nonpolitical." He credits Clinton, too, for his hands-off policy towards the Fed. A "Clinton-Greenspan mix," he claims, "of tight budgets and easier money contributed to the investment boom."

William Poole, President of the St. Louis Federal Reserve Bank, in his comments on Mankiw's paper agrees that Greenspan had proved that it is possible for the Fed to

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"manage policy quite actively," and that in the process he produced a "better outcome than would have been likely under a constant-money-growth rule."

In her comments about Mankiw's paper, Rivlin agrees with him that the "superb performance" of the economy in the 1990s cannot be attributed primarily to monetary policy. Instead, luck, the "absence of the demand and supply shocks that beset other recent decades" largely accounts for the performance of the economy in the 1990s.

In their paper, "Between Meltdown and Moral Hazard: The International Monetary and Financial Policies of the Clinton Administration," J. Bradford DeLong and Barry Eichengreen conclude that from a domestic point of view the Clinton administration's monetary and fiscal policy was "extraordinarily successful." They attribute this to the fact that deficit reduction created space for monetary ease that supported an investment-led recovery that ended a 20-year-long slowdown in productivity. DeLong and Eichengreen claim that because the international financial crises of this period followed a new pattern, they surprised policy-makers. As a result, there was too much dependence on "standard monetary and fiscal instruments in addressing crises whose roots were not fundamentally monetary or fiscal." They blame Robert Rubin and Lawrence Summers for not warning of the risks of international financial liberalization largely on "their belief that, if markets were not perfect, the alternatives were worse."

In "The Clinton Legacy for America's Poor," Rebecca M. Blank and David T. Ellwood claim that the Clinton years were a period of "remarkable social change" for the better because there was a dramatic increase in support for low-income working families. In his comment on this paper, Ron Haskins, who describes Clinton's social policies for the poor as one of the most successful in the nation's history, points out that Clinton lost control of welfare reform because he chose to table welfare reform in favor of health care reform, thereby providing Republicans with an opening to create their own legislation. Some liberals, he points out, still resent Clinton's support for the welfare reform law.

In his paper, "Tax Policy from 1990 to 2001," Eugene Steuerle, who served in various positions in the U.S. Treasury Department under four presidents, complains that "Traditional principles of equal treatment of equals, efficiency, and simplicity were often ignored, since tax policy was largely determined by budgetary, spending, and other concerns."

Panelist Nancy-Ann DeParle's description of Newhouse's paper as a "nice job of cataloging" is an apt description of this 1063-page tome as a whole. Its reading level is low enough that professors can assign readings from it to seniors majoring in economics whom they want to familiarize with recent economic history from the point of view of our mainstream economics elite. It can also be used as a reference work by today's journalists and tomorrow's historians. People familiar with a paper's subject matter are unlikely to find it enlightening.

CAROLE E. SCOTT
State University of West Georigia

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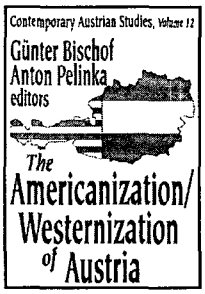

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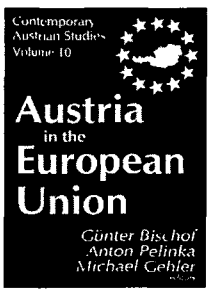

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THE PARADOX OF ASSET PRICING. BY PETER BOSSAERTS. PRINCETON, N.J.: PRINCETON UNIVERSITY PRESS, 2002.

A quantitative model defining the "intrinsic" value of a security has been and continues to be a primary ambition of many financial economists. Prior to the 1952 publication of Harry Markowitz's *Portfolio Selection* sophisticated participants in the world of financial speculation often relied on some form of fundamental or technical analysis to assess the investment merits of individual securities. Fundamental analysis is a process of gathering and analyzing information specific to determining the prospects of a security's future performance. It often requires gathering economic, industry, and company-specific data, and then utilizing this information to arrive at an appraisal of present or future price.

Perhaps the most important aspect of fundamental analysis is the increase in understanding gained by facilitating the process. Consistent with Austrian economic theory, it is this *understanding* of relevant information to the investment opportunity, formulated in the human mind, that aids in developing an estimate of future price.[1] Fundamental analysis is also consistent with Austrian economics because it represents an entrepreneurial process as the means of attaining the goal of profit by allocating investment capital to the most deserving companies. Markowitz's modern portfolio theory (MPT) and its resultant asset pricing models have attempted to displace fundamental analysis as the only "truly scientific" approach to investment analysis; disregarding the emphasis on individual security appraisal. In fact, most financial economists consider the development of MPT as defining the point at which the field of finance formally became a "true science."

Motivated by the quantitative logic of MPT, its foundational quantitative specifications of utility and risk aversion, Eugene Fama's formulation of an equilibrium-based efficient market hypothesis (EMH), and extensions utilizing aggregate market data such as Tobin's two-fund separation theorem, the advocates of economic positivism continued their search for an ultimate asset-pricing model. Sharpe (1964), Mossin (1966), and Litner (1965) brought us the first asset-pricing models based on EMH and MPT assumptions. Their work resulted in the capital asset pricing model (CAPM), which specifies the relationship between financial security return and risk (defined by the covariance of a security's historical return series with that of a representative risky market proxy). The relationship between risk and return specifies

[1] I have formulated the primary "role" of fundamental security analysis as "gaining understanding" based on cumulative readings of the works of Ludwig von Mises (*Human Action*, *Theory and History*, *The Ultimate Foundation of Economic Science*, and *Money, Method, and the Market Process*.)

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the appropriate market-clearing price (i.e., equilibrium). The CAPM contends that a security's required return has little or nothing to do with company and industry specific events for these sources of risk *are* simply immaterial as they are easily diversified away by investors, *all* of whom are assumed to "rationally" hold Markowitz efficient portfolios. While the academic advocates of the "scientific" approach to portfolio and investment management continue producing research that support the use of quantitative asset pricing models, results in real world applications have been dismal.

The failure of CAPM has fueled an ongoing debate over the correct paradigm of asset pricing. Disregarding (or simply ignoring) the writings of Ludwig von Mises, who emphatically states that prices are determined by subjective valuations of individuals and cannot be measured by cardinal numbers, diligent work has continued to (1) Salvage CAPM by reformulating it (i.e., intertemporal CAPM (Merton, 1973), and others), (2) Create new equilibrium models based on far different assumptions (arbitrage pricing theory [Ross, 1976]), or (3), Show that human behavioral constraints limit the ability of investors to act rationally and call for efforts to create a new or radically modified asset pricing paradigm. In spite of financial economists' best efforts, empirical and market tests of quantitative models provide little support for usefulness in real-world applications. Anyone familiar with the Misesian view of economic methodology (praxeology) could easily have predicted the failure of quantitative financial modeling attempts.

Utilizing a world of certainty (evenly rotating economy) for deducing crucial economic insights is a valuable tool for testing economic theory, but wrought with problems when considered descriptive of actual human actions. When economists attempt to supplant human based systems with artificial quantitative models they encounter a host of insurmountable methodological problems due to the variability and complexity of past, present, and future economic environments. Nevertheless, financial economists persist in assuming that the real world can be replicated in asset pricing models. What are especially troubling are the methodological violations in formulating these so-called theories of asset pricing in spite of the continued failures of such models in applications. Ludwig von Mises is well known for his sharp criticisms of the methods employed by the proponents of positivism and empiricism. The utilization of methodological collectivism versus methodological individualism, the problems of constructing indexes to measure aggregate activity, confusion over the proper application of class versus case probability, and the overall mechanistic approach of quantitative modeling violate the Misesian prescription of praxeology as the proper study of purposeful human action.

Unfortunately, Peter Bossaerts' text, *The Paradox of Asset Pricing*, offers no relief from past use of flawed methodologies. Bossaerts is professor of finance and director of the Laboratory for Experimental Finance at the California Institute of Technology. His manuscript is aimed at the academic/professional market and is based on a collection of his lectures, working papers, and published research. Based on the title and preface, one may assume that Bossaerts is launching a full-scale attack on the use of positivism and econometric empiricism in past attempts to study asset pricing; this view is quickly extinguished by the time the reader gets to page xi of the Preface. In terms of general methodology, he utilizes the same techniques and measures that most Austrian economists reject.

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The essence of Bossaerts' text is an attack on the efficient market hypothesis (EMH) as the basic premise of asset pricing models. Most Austrian economists would be sympathetic with attempts to overthrow the application of an artificial construct such as EMH as a description of the way security prices assimilate and reflect relevant information. In his text Bossaerts leaves little changed in terms of the ambitious use of mathematical proofs and general support for quantitative modeling. He begins by providing an overview of the commonly used quantitative modeling approach to asset pricing, then proceeds with evidence questioning the validity of EMH as a foundational assumption of asset pricing models. Following the deluge of mathematical calculations and models (which permeate the entire text) he eventually shows the reader the weakness of results obtained from empirical tests of asset pricing models based on the assumption of EMH. Bossaerts also asserts that weak empirical test results of asset pricing models may be attributable to the complexity of research utilizing historical data; a reasonable proposition from the view of most Austrian economists. He quickly oversteps the bounds of reason by suggesting that a better empirical testing method is to create "laboratory experiments" in the spirit of game theory.

Thus, Bossaerts creates a laboratory experiment to examine the dynamics of asset price evolution. Bossaerts' laboratory experiment is basically a simulated trading game in which participants are handed a quantity of money, and told to bid for three securities (two risky and one risk free) initially set at different price levels. Participants are given no prior information about the securities. They are not given information about ending prices but are provided access to information concerning the historical trades of others in the subject group as the game progresses. Participants get to keep their ending wealth at game's end. I will not go into the details of his quasi-experimental design except to say that the trading groups participating hardly represented breadth and depth of sample (two groups each composed of MBA students from Yale, Stanford, and UCLA and one group from Caltech); there are additional design questions as well. Needless to say, most Austrian economists would consider the attempt to perform an economic "laboratory experiment" beyond the bounds of appropriate methodology. In spite of his questionable experimental method, results show that near equilibrium prices generally obtain (although in some cases prices drift in and out of near equilibrium). He attributes anomalies in the results to the irrational speculation of subjects. Results appear to support asset-pricing models but lead to concerns about EMH, especially the requirement that market participants hold a set of correct initial beliefs.

Based on knowledge gained from his "experiment," Bossaerts proceeds to develop a new hypothesis of market behavior he labels the efficiently learning market (ELM). By relaxing the EMH assumption of correct initial beliefs or "priors," he provides evidence that markets tend toward equilibrium based on a subjective learning process as participants compete against each other to optimize ending wealth. Admittedly, ELM has a greater logical appeal than EMH because it encompasses more realistic characteristics of human behavior. He then attempts to verify the ELM hypothesis by constructing an empirical study utilizing historical data from initial public offerings of common stocks. His results are somewhat suspect (again, there are contradictions) but generally supportive of ELM. In any respect, he does show that the ELM "fits the historical record better than EMH." As Bossaerts asserts, "it appears to be more appropriate to model financial markets in the image of a Bayesian learner who may at times hold mistaken expectations." This is perhaps a lot of work to prove the obvious for is

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there really anything new in proving that individuals can err when forming expectations of future events?

The Paradox of Asset Pricing is more revealing of paradoxes than the author may have intended. The author is somewhat successful in refuting EMH and the assumption of equilibrium conditions *instantaneously* obtaining in asset prices. To that extent, Austrian economists may applaud. From the perspective of the praxeological method as the only acceptable methodology for the study of human action, the reader is treated to a host of fallacies, absurdities, and contradictions that explain the ineffectiveness and wholesale waste of resources prevalent in most modern-day financial economic research. It is obvious that the author (as is likely the case of most financial economists) is unsympathetic to Austrian methodology by his repeated attempts to "brute force" common sense into the language of mathematics. It cannot be assumed that he is ignorant of the Austrian School for in one sentence he does display some familiarity by stating "There is a school of thought in economics, the Neo-Austrian school, that rejects the very idea that markets equilibrate" (p.16).

If anything, the Bossaerts text provides a good example of "state of the art" research in current financial economic literature. His efforts expand on a line of research that totally rejects methodological individualism. For example, there are no references to the role of a firm's fundamentals or any reference to the actions of the *individual* in asset pricing. The work of modern financial theorists and econometricians routinely reject the study of individual cases to gain knowledge and understanding. In their view, all relevant information is encompassed in collective or aggregate parameters.

Most business schools and economics departments indoctrinate their students with the modern methods of quantitative modeling and econometrics like that represented in *The Paradox of Asset Pricing*. Fundamental analysis is considered gauche. Is it any wonder that today's graduates from the so-called "best" business schools who establish careers on Wall Street fail to identify irregularities in financial statements and grossly err with their recommendations?

RICHARD C. GRIMM
Grove City College

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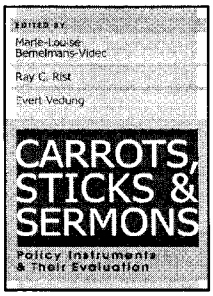

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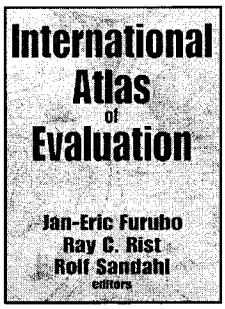

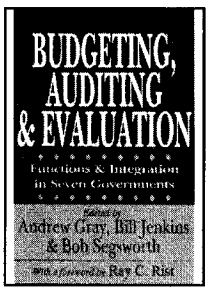
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